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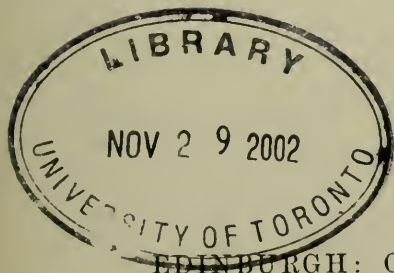
THE TRANSACTIONS

OF THE

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

VOL. III.—NEW SERIES.

SESSION 1883-84.



EDINBURGH: OLIVER AND BOYD,
PUBLISHERS TO THE SOCIETY.

1884.

PRINTED BY OLIVER AND BOYD, TWEEDDALE COURT, EDINBURGH.



PREFACE.

THE present Volume is the *Third* of the *New Series*, and contains a record of the work done during the past Session.

That work, as hitherto, embraces the communication of Original Papers; the exhibition of Patients, illustrating rare and interesting forms of disease; and the exhibition of Pathological and other specimens, so essential to the proper understanding of the morbid changes which take place in the human body; and, in addition, contains an important Discussion on Catheter Fever, introduced by Sir Andrew Clark, Bart., London.

It is believed that the publication of the Transactions in this permanent form will prove a valuable contribution to medical literature, will encourage the members to take a more active part in the work of the Society, and will tend in no small degree to increase the influence and usefulness of the Medico-Chirurgical Society of Edinburgh.

WILLIAM CRAIG,
Editor.

September 1884.

Medico-Chirurgical Society of Edinburgh.

INSTITUTED 2ND AUGUST 1821.

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| | * William Allan Jamieson, M.D., F.R.C.P. Ed., 26 Rutland Street, | 1876 |
| 105 | Alexander Moir, L.R.C.P. and S. Ed., 30 Buccleuch Place, | 1876 |
| | * Charles H. Thatcher, F.R.C.S. Ed., 13 Albany Street, | 1876 |
| | * William Ziegler, M.D., F.R.C.P. Ed., 47 George Square, | 1876 |
| | George Herbert Bentley, L.R.C.P. and S. Ed., Kirkliston, | 1877 |
| | Surgeon-Major William T. Black, M.D., F.R.C.S. Ed., 2 George Square, | 1877 |
| 110 | * John Brown Buist, M.D., F.R.C.P. Ed., 1 Clifton Terrace, | 1877 |
| | William Watson Campbell, M.D., F.R.C.P. Ed., Duns, | 1877 |
| | * George Hunter, M.D., F.R.C.S. Ed., Linlithgow, | 1877 |
| | Alexander James, M.D., F.R.C.P. Ed., 11 Albyn Place, <i>Secretary</i> , | 1877 |
| | * James Jamieson, M.D., F.R.C.S. Ed., 43 George Square, | 1877 |
| 115 | Charles Watson MacGillivray, M.D., F.R.C.S. Ed., 11 Rutland Street, <i>Secretary</i> , | 1877 |
| | * Thomas Rutherford Ronaldson, M.B., F.R.C.P. Ed., 18 Bruntsfield Place, | 1877 |
| | George D. Smith, M.D., M.R.C.P. Ed., The Priory, Newhaven Road, | 1877 |
| | James Stitt Thomson, M.R.C.P. Ed., Dalkeith, | 1877 |
| | Bryan Charles Waller, M.B., F.R.C.S. Ed., Masongill House, Carnforth, | 1877 |
| 120 | * John Graham Brown, M.D., F.R.C.P. Ed., 16 Ainslie Place, | 1878 |
| | Alexander Robert Coldstream, M.B., F.R.C.S. Ed., Florence, Italy, | 1878 |
| | * Joseph Montagu Cotterill, M.B., F.R.C.S. Ed., 24 Melville Street, | 1878 |
| | George Mackay, M.B., F.R.C.S. Ed., 2A Gilmore Place, | 1878 |
| | David Menzies, M.B., F.R.C.S. Ed., 21 Rutland Square, | 1878 |
| 125 | James Allan Philip, M.B., Monte Carlo, Italy, | 1878 |
| | John Shand, M.D., F.R.C.P. Ed., 34 Albany Street, | 1878 |
| | Johnson Symington, M.B., F.R.C.S. Ed., 10 Warrender Park Crescent, | 1878 |
| | William Barrie Dow, M.D., F.R.C.S. Ed., Dunfermline, | 1879 |
| | John Fraser, M.B., M.R.C.P. Ed., 31 Regent Terrace, | 1879 |
| 130 | Richard Freeland, M.B., C.M., Broxburn, | 1879 |
| | James Allan Gray, M.D., F.R.C.P. Ed., 107 Ferry Road, | 1879 |
| | Peter M'Bride, M.D., F.R.C.P. Ed., 16 Chester Street, | 1879 |
| | William Stewart, M.B., C.M., Kirkwall, | 1879 |
| | Charles H. Fasson, Dep. Surgeon-General, Royal Infirmary, | 1879 |
| 135 | A. D. Leith Napier, M.D., C.M., Dunbar, | 1879 |
| | Andrew Fleming, M.D., Dep. Surgeon-General, 8 Napier Road, | 1880 |
| | Thomas Duddingston Wilson, M.B., F.R.C.S. Ed., 5 West Newington Terrace, | 1880 |
| | T. Fair H. Spence, M.B., C.M., 8 Castle Terrace, | 1881 |
| | George Leslie, M.B., C.M., Old Manse, Falkirk, | 1881 |
| 140 | Robert Lawson, M.D., C.M., 24 Mayfield Terrace, | 1881 |
| | John Hutton Balfour, M.B., C.M., Portobello, | 1881 |
| | Alexander Hugh Freeland Barbour, M.D., F.R.C.P. Ed., 50 Queen Street, | 1881 |
| | George Alexander Gibson, M.D., F.R.C.P. Ed., 17 Alva Street, | 1881 |
| | William Badger, M.B., C.M., Penicuik, | 1882 |
| 145 | Alexander Fergusson, M.D., F.R.C.S. Ed., Peebles, | 1882 |
| | Alexander Matthew, F.R.C.S. Ed., Corstorphine, | 1882 |
| | John Archibald, M.B., F.R.C.S. Ed., Lynton House, Brixton Rise, Brixton, London, S.W., | 1882 |
| | James Maxwell Ross, M.B., F.R.C.S. Ed., 112 Gilmore Place, | 1882 |
| | John Carlyle Johnstone, M.B., C.M., Morningside Asylum, | 1882 |
| 150 | James Rutherford Morison, M.B., F.R.C.S. Ed., Hartlepool, | 1882 |
| | Roderick Maclaren, M.D., 23 Portland Square, Carlisle, | 1882 |
| | W. Wotherspoon Ireland, M.D., Prestonpans, | 1883 |
| | Francis Mitchell Caird, M.B., F.R.C.S. Ed., 3 Torphichen St, | 1883 |

| | | Date of Admission. |
|-----|---|-----------------------|
| | F. W. Dyce Fraser, M.D., F.R.C.P. Ed., 20 Chester Street, . . . | 1883 |
| 155 | James Longmuir, M.D., Bathgate, . . . | 1883 |
| | Robert Henry Blaikie, M.B., C.M., 2 Newington Terrace, . . . | 1883 |
| | R. M'Kenzie Johnstone, M.B., C.M., 5 Rutland Square, . . . | 1883 |
| | Charles Walker Cathcart, M.B., F.R.C.S. Ed., 44 Melville Street, . . . | 1883 |
| | Alexander Bruce, M.B., F.R.C.P. Ed., 13 Alva Street, . . . | 1883 |
| 160 | Andrew Semple, M.D., F.R.C.S. Ed., Dep. Surgeon-General, 8 Abercromby Place, . . . | 1883 |
| | William Hy. Shirreff, M.B., C.M., 29 Castle Terrace, . . . | 1883 |
| | John Lyon Wilson, L.R.C.P. Ed., 44 South Clerk Street, . . . | 1883 |
| | Donald MacRaid, F.R.C.S. Ed., Greenock, . . . | 1883 |
| | Henry Newcombe, M.D., F.R.C.S. Ed., 5 Dalrymple Crescent, . . . | 1883 |
| 165 | Francis Troup, M.D., M.R.C.P. Ed., 1 Minto Street, . . . | 1883 |
| | Russell Elliott Wood, M.B., C.M., 9 Darnaway Street, . . . | 1883 |
| | John Macdonald Brown, M.B., F.R.C.S. Ed., 6 Atholl Place, . . . | 1883 |
| | James William Beeman Hodsdon, M.D., M.R.C.P. Ed., 30 Walker Street, . . . | 1883 |
| | John Haddon, M.D., C.M., Honeyburn, Hawick, . . . | 1883 |
| 170 | Germon Sims Woodhead, M.D., F.R.C.P. Ed., 6 Marchhall Crescent, . . . | 1883 |
| | Thomas Francis Spittal Caverhill, M.B., F.R.C.P. Ed., 8A Aber- cromby Place, . . . | 1883 |
| | Robert Alexander Lundie, M.B., B.Sc., 36 Warrender Park Road, . . . | 1883 |
| | Arthur W. Hare, M.B., C.M., 21 Ainslie Place, . . . | 1883 |
| | Edwin Baily, M.B., C.M., Oban, . . . | 1883 |
| 175 | Alexander Black, M.B., F.R.C.P. Ed., 8 Clarence Street, . . . | 1883 |
| | Harry Melville Dunlop, M.B., C.M., Middlefield House, Leith Walk, . . . | 1883 |
| | George Andreas Berry, M.B., F.R.C.S. Ed., 23 Rutland Street, . . . | 1883 |
| | Hamilton Wylie, M.B., C.M., 3 George Place, . . . | 1883 |
| | James Bennet, M.B., C.M., 49 Charlotte Street, Leith, . . . | 1883 |
| 180 | Arthur Douglas Webster, M.D., M.R.C.P. Ed., 10 West New- ington Terrace, . . . | 1883 |
| | Robert William Philip, M.B., C.M., 12 Hope Street, . . . | 1883 |
| | William M'Neil, M.D., Stranraer, . . . | 1883 |
| | Joseph Carne Ross, M.D., F.R.C.P. Ed., Penzance, . . . | 1884 |
| | William Russell, M.B., 33 Gayfield Square, . . . | 1884 |
| 185 | George Dickson, M.D., F.R.C.S. Ed., 9 India Street, . . . | 1884 |
| | Thomas Wyld Pairman, L.R.C.P. & S. Ed., Stogumber, Taunton, . . . | 1884 |
| | Alexander Thom, jr., M.D., C.M., Crieff, . . . | 1884 |
| | Hugh Logan Calder, M.B., C.M., 42 Leith Walk, . . . | 1884 |
| | James Craig Balfour, L.R.C.P. & S. Ed., 4 Newington Terrace, . . . | 1884 |
| 190 | Frederick Anastasius Saunders, L.R.C.P. & S. Ed., Denburn, Crail, . . . | 1884 |

NON-RESIDENT.

| | | |
|-----|---|------|
| | Arthur Edward Turnour, M.D., M.R.C.S. Eng., <i>Denbigh</i> , . . . | 1843 |
| | George Skene Keith, M.D., F.R.C.P. Ed., <i>Currie</i> , . . . | 1845 |
| | W. Judson Van Someren, M.D., L.R.C.S. Ed., <i>Redhill, Surrey</i> , . . . | 1845 |
| | W. Ord M'Kenzie, M.D., L.R.C.S. Ed., <i>London</i> , . . . | 1845 |
| 195 | W. Chalmers Fowler, M.D., M.R.C.S. Eng., <i>London</i> , . . . | 1847 |
| | Veitch Sinclair, L.R.C.P. and S. Ed., <i>London</i> , . . . | 1850 |
| | Thomas Sandem Watson, M.D., M.R.C.P. L., <i>Bath</i> , . . . | 1852 |
| | Andrew Graham, M.D., Fleet Surgeon, R.N., . . . | 1853 |
| | Archibald Hall, M.D., <i>Montreal</i> , . . . | 1853 |
| 200 | John Traill, F.R.C.S. Ed., <i>Arbroath</i> , . . . | 1853 |
| | William H. Lowe, M.D., F.R.C.P. Ed., <i>Wimbledon</i> , . . . | 1854 |

| | | Date of Admission. |
|-----|--|--------------------|
| | W. Overend Priestly, M.D., LL.D., F.R.C.P. Ed., <i>London</i> , | 1854 |
| | Horatio Robinson Storer, M.D., <i>Newport, Rhode Island, U.S.</i> , | 1855 |
| | James C. Howden, M.D., <i>Montrose</i> , | 1856 |
| 205 | Thomas Skinner, M.D., L.R.C.S. Ed., <i>London</i> , | 1856 |
| | J. Ivor Murray, M.D., F.R.C.S. Ed., <i>Scarboro'</i> , | 1857 |
| | Professor William Smout Playfair, M.D., F.R.C.P.L., <i>London</i> , | 1857 |
| | Andrew Scott Myrtle, M.D., L.R.C.S. Ed., <i>Harrogate</i> , | 1858 |
| | W. Alexander Francis Browne, F.R.C.S. Ed., <i>Dumfries</i> , | 1858 |
| 210 | Robert Foulis, M.D., F.R.C.S. Ed., <i>Cupar-Fife</i> , | 1859 |
| | Francis Robertson Macdonald, M.D., <i>Inveraray</i> , | 1859 |
| | Professor John Young, M.D., <i>University of Glasgow</i> , | 1859 |
| | J. Cecil Phillippo, M.D., <i>Kingston, Jamaica</i> , | 1860 |
| | Norman Bethune, M.D., F.R.C.S. Ed., <i>Toronto</i> , | 1861 |
| 215 | T. Spencer Cobbold, M.D., F.R.S., <i>London</i> , | 1861 |
| | Peter Gordon, L.R.C.P. and S. Ed., <i>Juniper Green</i> , | 1861 |
| | Professor William Stephenson, M.D., F.R.C.S. Ed., <i>Aberdeen</i> , | 1861 |
| | George Thin, M.D., L.R.C.S. Ed., <i>London</i> , | 1861 |
| | David Yellowlees, M.D., F.F.P.S. Glasg., <i>Glasgow</i> , | 1862 |
| 220 | Professor Arthur Gamgee, M.D., F.R.C.P. Ed., F.R.S., <i>Manchester</i> , | 1863 |
| | William M'Culloch Watson, M.D., <i>Montrose</i> , | 1863 |
| | Professor John Cleland, M.D., LL.D., <i>The University, Glasgow</i> , | 1864 |
| | R. B. Finlay, M.D., <i>Middle Temple, London</i> , | 1864 |
| | Stanley Lewis Haynes, M.D., M.R.C.S. Eng., <i>Malvern</i> , | 1864 |
| 225 | Francis D. A. Skae, M.D., <i>Lerwick</i> , | 1864 |
| | James Watt Black, M.D., F.R.C.P.L., <i>London</i> , | 1865 |
| | David Brodie, M.D., <i>Canterbury</i> , | 1865 |
| | Thomas Sheriff, L.R.C.P. and S. Ed., <i>1 Chalmers Crescent</i> , | 1867 |
| | Lauchlan Aitken, M.D., C.M., <i>Rome</i> , | 1868 |
| 230 | Peter Maury Deas, M.B., L.R.C.S. Ed., <i>Macclesfield</i> , | 1868 |
| | J. G. Sinclair Coghill, M.D., F.R.C.P. Ed., <i>Ventnor</i> , | 1870 |
| | Professor J. G. M'Kendrick, M.D., F.R.C.P. Ed., <i>University, Glasgow</i> , | 1870 |
| | Lawson Tait, F.R.C.S. Ed. and Eng., <i>Birmingham</i> , | 1870 |
| | J. William Eastwood, M.D., M.R.C.P.L., <i>Darlington</i> , | 1871 |
| 235 | James Johnston, M.D., L.R.C.S. Ed., <i>Shanghai</i> , | 1871 |
| | Professor J. Bell Pettigrew, M.D., F.R.C.P. Ed., <i>University of St Andrews</i> , | 1873 |
| | John Smith, | 1873 |
| | Frederick William Barry, M.D., B.Sc., <i>London</i> , | 1874 |
| | Dr Groesbeck, <i>Cincinnati</i> , | 1875 |
| 240 | John Aymers Macdougall, M.D., F.R.C.S. Ed., <i>Carlisle</i> , | 1875 |
| | Thomas John MacLagan, M.D., M.R.C.P.L., <i>London</i> , | 1875 |
| | Professor F. S. B. de Chaumont, M.D., F.R.C.S. Ed., <i>Nelley</i> , | 1876 |
| | Professor David James Hamilton, M.B., F.R.C.S. Ed., <i>Aberdeen University</i> , | 1876 |
| | J. Moolman, M.B., C.M., <i>Cape of Good Hope</i> , | 1877 |
| 245 | Robert Somerville, M.D., L.R.C.S. Ed., <i>Galashiels</i> , | 1877 |
| | Graham Steell, M.D., M.R.C.P.L., <i>Manchester</i> , | 1877 |
| | John Brown, M.D., F.R.C.S. Eng., <i>Burnley</i> , | 1878 |
| | Thomas Inglis, F.R.C.P. Ed., <i>Lincoln</i> , | 1878 |
| | Walter Weir, M.B., F.R.C.P. Ed., <i>London</i> , | 1879 |
| 250 | Keith Norman Macdonald, M.D., F.R.C.P. Ed., <i>Cupar-Fife</i> , | 1880 |
| | John Home Hay, M.D., M.R.C.S. Eng., <i>Alloa</i> , | 1880 |
| | John Mackay, M.D., L.R.C.S. Ed., <i>Aberfeldy</i> , | 1881 |

ORDINARY MEMBERS,

ARRANGED ALPHABETICALLY.

RESIDENT.

| | | Date of Admission. |
|----|--|-----------------------|
| | Dr J. O. Affleck, 38 Heriot Row, | 1871 |
| | Dr James Andrew, 2 Atholl Crescent, | 1869 |
| | Professor Annandale, 34 Charlotte Square, | 1863 |
| | Dr John Archibald, London, | 1882 |
| 5 | Dr W. Badger, Penicuik, | 1882 |
| | Dr J. Johnson Bailey, Marple, Cheshire, | 1874 |
| | Dr Edwin Baily, Oban, | 1883 |
| | Dr Andrew Balfour, Portobello, | 1874 |
| | Dr J. H. Balfour, Portobello, | 1881 |
| 10 | Dr G. W. Balfour, 17 Walker Street, | 1874 |
| | Dr James Craig Balfour, 4 Newington Terrace, | 1884 |
| | Dr Thomas Balfour, 51 George Square, | 1856 |
| | Dr Ballantyne, Dalkeith, | 1873 |
| | Dr A. H. Freeland Barbour, 50 Queen Street, | 1881 |
| 15 | Joseph Bell, Esq., 2 Melville Crescent, | 1862 |
| | Dr James Bennet, 49 Charlotte Street, Leith, | 1883 |
| | G. H. Bentley, Esq., Kirkliston, | 1877 |
| | Dr G. A. Berry, 23 Rutland Street, | 1883 |
| | Dr James S. Beveridge, 8 Eildon Street, | 1861 |
| 20 | Dr Bishop, | 1872 |
| | Dr Alexander Black, 8 Clarence Street, | 1883 |
| | Dr W. T. Black, 2 George Square, | 1877 |
| | Dr Robert H. Blaikie, 2 Newington Terrace, | 1883 |
| | Dr Bleloch, 13 Brougham Street, | 1871 |
| 25 | Dr Brakenridge, 10 St Colme Street, | 1865 |
| | Dr Byrom Bramwell, 23 Drumsheugh Gardens, | 1876 |
| | Dr J. Graham Brown, 16 Ainslie Place, | 1878 |
| | Dr J. Macdonald Brown, 6 Atholl Place, | 1883 |
| | William Brown, Esq., 25 Dublin Street, | 1821 |
| 30 | Dr Alexander Bruce, 13 Alva Street, | 1883 |
| | Dr Robert Bruce, 12 York Place, | 1858 |
| | Dr Buist, 1 Clifton Terrace, | 1877 |
| | Dr Burn, 45 Lauriston Place, | 1847 |
| | Dr Cadell, 5 Castle Terrace, | 1870 |
| 35 | Dr Francis M. Caird, 8 Torphichen Street, | 1883 |
| | Dr H. L. Calder, 42 Leith Walk, | 1884 |
| | Dr W. Watson Campbell, Duns, | 1877 |
| | Dr Cappie, 47 Lauriston Place, | 1855 |
| | Dr J. Carmichael, 22 Northumberland Street, | 1870 |
| 40 | Dr C. W. Cathcart, 44 Melville Street, | 1883 |
| | Dr T. F. S. Caverhill, 8A Abercromby Place, | 1884 |
| | Professor John Chiene, 26 Charlotte Square, | 1867 |
| | Dr Church, 36 George Square, | 1876 |
| | Dr Clouston, Morningside Asylum, | 1871 |
| 45 | Dr A. R. Coldstream, Florence, | 1878 |
| | Dr John Connel, Peebles, | 1876 |
| | Dr Cotterill, 24 Melville Street, | 1878 |
| | Dr William Craig, 7 Lothian Road, | 1869 |
| | Dr Halliday Croom, 25 Charlotte Square, | 1870 |
| 50 | Dr Cumming, 18 Ainslie Place, | 1845 |
| | Dr R. J. B. Cunynghame, 6 Walker Street, | 1868 |
| | Dr Archibald Dickson, 11 Royal Circus, | 1871 |
| | Dr George Dickson, 9 India Street, | 1884 |
| | Dr Halliday Douglas, 30 Melville Street, | 1842 |
| 55 | Dr William B. Dow, Dunfermline, | 1879 |
| | Dr John Duncan, 8 Ainslie Place, | 1865 |
| | Dr Kirk Duncanson, 22 Drumsheugh Gardens, | 1870 |

| | | Date of Admission. |
|-----|---|-----------------------|
| | Dr H. M. Dunlop, Middlefield House, | 1883 |
| | Dr Dunsinure, 53 Queen Street, | 1841 |
| 60 | Dr J. Dunsinure, jun., 53 Queen Street, | 1872 |
| | C. H. Fasson, Esq., Dep. Surg.-Gen., Royal Infirmary, | 1879 |
| | Dr Alexander Fergusson, Peebles, | 1882 |
| | Dr Finlay, St Colms, Lennox Row, | 1847 |
| | Dr W. A. Finlay, St Helens, Russell Place, | 1874 |
| 65 | Dr Andrew Fleming, 8 Napier Road, | 1880 |
| | Dr Foulis, 34 Heriot Row, | 1875 |
| | Dr F. W. Dyce Fraser, 20 Chester Street, | 1883 |
| | Dr John Fraser, 31 Regent Terrace, | 1879 |
| | Professor Thomas R. Fraser, 37 Melville Street, <i>Vice-President</i> , | 1865 |
| 70 | Dr R. Freeland, Broxburn, | 1879 |
| | Dr Garland, 35 Charlotte Street, Leith, | 1873 |
| | Dr G. A. Gibson, 17 Alva Street, | 1881 |
| | Dr James D. Gillespie, 10 Walker Street, | 1850 |
| | G. R. Gilruth, Esq., 67 York Place, | 1869 |
| 75 | Dr J. Allan Gray, 107 Ferry Road, | 1879 |
| | Dr Greig, 38 Coates Gardens, | 1854 |
| | Dr R. H. Gunning, London, | 1854 |
| | Dr John Haddon, Honeyburn, Hawick, | 1883 |
| | Dr Haldane, 22 Charlotte Square, | 1853 |
| 80 | Dr A. W. Hare, 21 Ainslie Place, | 1883 |
| | Dr John Henderson, 7 John's Place, Leith, | 1848 |
| | Dr J. W. B. Hodson, 30 Walker Street, | 1883 |
| | Dr George Hunter, Linlithgow, | 1877 |
| | Dr James Hunter, 18 Abergromby Place, | 1850 |
| 85 | Dr Husband, 28 Clarence Street, | 1850 |
| | Francis B. Imlach, Esq., 48 Queen Street, | 1843 |
| | Dr Archibald Inglis, 33 Albany Street, | 1827 |
| | Dr W. Wotherspoon Ireland, Prestonpans, | 1883 |
| | Dr James, 11 Albyn Place, <i>Secretary</i> , | 1877 |
| 90 | Dr W. Allan Jamieson, 26 Rutland Street, | 1876 |
| | Dr James Jamieson, 43 George Square, | 1877 |
| | Dr R. M'Kenzie Johnston, 5 Rutland Square, | 1883 |
| | Dr J. Carlyle Johnstone, Morningside Asylum, | 1882 |
| | Dr Kalley, 9 Tipperlinn Road, | 1860 |
| 95 | Dr Keiller, 21 Queen Street, | 1845 |
| | Dr Thomas Keith, 2 North Charlotte Street, | 1852 |
| | Dr Robert Lawson, 24 Mayfield Terrace, | 1881 |
| | Dr George Leslie, Falkirk, | 1881 |
| | Dr Linton, 60 George Square, | 1863 |
| 100 | Dr Littlejohn, 24 Royal Circus, <i>President</i> , | 1853 |
| | Dr James Longmuir, Bathgate, | 1883 |
| | Dr Lucas, Dalkeith, | 1875 |
| | Dr R. A. Lundie, 36 Warrender Park Road, | 1883 |
| | Dr P. M'Bride, 16 Chester Street, | 1879 |
| 105 | Dr Angus Macdonald, 29 Charlotte Square, | 1866 |
| | John M'Gibbon, Esq., 55 Queen Street, | 1868 |
| | Dr MacGillivray, 11 Rutland Street, <i>Secretary</i> , | 1877 |
| | Dr G. Mackay, 2A Gilmore Place, | 1878 |
| | Professor MacLagan, 28 Heriot Row, | 1834 |
| 110 | Dr P. H. MacLaren, 1 Drumsheugh Gardens, | 1868 |
| | Dr Roderick M'Laren, Carlisle, | 1882 |
| | Dr William M'Neil, Stranraer, | 1883 |
| | Dr Donald MacRaid, Greenock, | 1883 |
| | Dr A. Matthew, Corstorphine, | 1882 |
| 115 | Dr D. Menzies, 21 Rutland Square, | 1878 |
| | Dr W. Menzies, 3 Lothian Road, | 1847 |
| | A. G. Miller, Esq., 11 Walker Street, <i>Treasurer</i> , | 1866 |
| | Dr Arthur Mitchell, 34 Drummond Place, | 1859 |
| | Dr Moinet, 20 Coates Crescent, | 1870 |
| 120 | Dr Moir, 52 Castle Street, | 1886 |

| | | Date of Admission. |
|-----|---|-----------------------|
| | Alexander Moir, Esq., 30 Buccleuch Place, | 1876 |
| | Dr J. Rutherford Morison, Hartlepool, | 1882 |
| | Dr Claud Muirhead, 30 Charlotte Square, | 1866 |
| | Dr A. D. Leith Napier, Dunbar, | 1879 |
| 125 | Dr H. Newcombe, 5 Dalrymple Crescent, | 1883 |
| | Dr Niven, 110 Lauriston Place, | 1845 |
| | Dr P. Orphoot, 113 George Street, | 1865 |
| | Dr T. W. Fairman, Stogumber, | 1884 |
| | Dr Paterson, 15 Merchiston Park, | 1837 |
| 130 | Dr Pattison, 26 London Street, | 1842 |
| | Dr Peddie, 15 Rutland Street, | 1842 |
| | Dr J. A. Philip, Italy, | 1878 |
| | Dr R. W. Philip, 12 Hope Street, | 1883 |
| | Dr Playfair, 25 Rutland Street, | 1874 |
| 135 | J. D. Pridie, Esq., 6 Minto Street, | 1857 |
| | Dr Rattray, Portobello, | 1874 |
| | Dr James Ritchie, 14 Charlotte Square, | 1873 |
| | Dr R. Peel Ritchie, 1 Melville Crescent, | 1861 |
| | Dr Argyll Robertson, 18 Charlotte Square, | 1861 |
| 140 | Dr Ronaldson, 18 Bruntsfield Place, | 1877 |
| | Dr J. Maxwell Ross, 112 Gilmore Place, | 1882 |
| | Dr Joseph C. Ross, Penzance, | 1884 |
| | Dr William Russell, 33 Gayfield Square, | 1884 |
| | Professor Rutherford, 14 Douglas Crescent, | 1874 |
| 145 | Dr F. A. Saunders, Denburn, Crail, | 1884 |
| | Dr Andrew Semple, 8 Abererombie Place, | 1883 |
| | Dr John Shand, 34 Albany Street, | 1878 |
| | C. H. E. Sheaf, Esq., Barrow-in-Furness, | 1871 |
| | Dr W. H. Shirreff, 29 Castle Terrace, | 1883 |
| 150 | Dr J. Sibbald, 3 St Margaret's Road, | 1858 |
| | Dr James Sidey, 20 Heriot Row, | 1849 |
| | Professor Simpson, 52 Queen Street, | 1857 |
| | Dr A. J. Sinclair, 21 Northumberland Street, | 1873 |
| | Dr Andrew Smart, 20 Charlotte Square, | 1864 |
| 155 | Dr G. D. Smith, The Priory, Newhaven Road, | 1877 |
| | Dr John Smith, 11 Wemyss Place, | 1856 |
| | Dr T. F. H. Spence, 8 Castle Terrace, | 1881 |
| | Professor Grainger Stewart, 19 Charlotte Square, | 1860 |
| | Dr W. Stewart, Kirkwall, | 1879 |
| 160 | Dr John Strachan, Dollar, | 1867 |
| | Dr James Struthers, 39 Charlotte Street, Leith, | 1849 |
| | Dr Johnson Symington, 10 Warrender Park Crescent, | 1878 |
| | Dr W. Taylor, 12 Melville Street, | 1870 |
| | C. H. Thatcher, Esq., 13 Albany Street, | 1876 |
| 165 | Dr Alexander Thom, jr., Crieff, | 1884 |
| | Dr Alexander Thomson, 14 Rankeillor Street, | 1849 |
| | J. Stitt Thomson, Esq., Dalkeith, | 1877 |
| | Dr Francis Troup, 1 Minto Street, | 1883 |
| | Dr Batty Tuke, 20 Charlotte Square, <i>Vice-President</i> , | 1854 |
| 170 | Professor Turner, 6 Eton Terrace, | 1858 |
| | Dr R. S. Turner, Keith, | 1867 |
| | Dr Underhill, 8 Coates Crescent, | 1872 |
| | William Walker, Esq., 47 Northumberland Street, | 1841 |
| | B. C. Waller, Esq., M.B., Carnforth, | 1877 |
| 175 | Dr P. H. Watson, 16 Charlotte Square, | 1856 |
| | Dr W. Watson, Midealder, | 1862 |
| | Dr A. D. Webster, 10 West Newington Terrace, | 1883 |
| | Dr Graham Weir, 36 Heriot Row, | 1837 |
| | Dr Williamson, 53 Charlotte Street, Leith, | 1849 |
| 180 | Dr David Wilson, 12 Dean Terrace, <i>Vice-President</i> , | 1844 |
| | J. L. Wilson, Esq., 44 South Clerk Street, | 1883 |
| | Dr T. D. Wilson, 5 West Newington Terrace, | 1880 |
| | Dr Russell E. Wood, 9 Darnaway Street, | 1883 |

| | | Date of Admission. |
|-----|--|--------------------|
| | Dr G. Sims Woodhead, 6 Marchhall Crescent, | 1883 |
| 185 | Dr Strehill Wright, Southport, | 1871 |
| | Dr Hamilton Wylie, 3 George Place, | 1883 |
| | Dr Wylie, 1 Melville Street, | 1868 |
| | Dr James Young, 14 Ainslie Place, | 1853 |
| | Dr P. A. Young, 25 Manor Place, | 1870 |
| 190 | Dr Ziegler, 47 George Square, | 1876 |

NON-RESIDENT.

| | | |
|-----|--|------|
| | Dr Lanchlan Aitken, <i>Rome</i> , | 1868 |
| | Dr F. W. Barry, <i>London</i> , | 1874 |
| | Dr Bethune, <i>Toronto</i> , | 1861 |
| | Dr J. W. Black, <i>London</i> , | 1865 |
| 195 | Dr Brodie, <i>Canterbury</i> , | 1865 |
| | Dr John Brown, <i>Burnley</i> , | 1878 |
| | W. A. F. Browne, Esq., <i>Dumfries</i> , | 1858 |
| | Professor F. S. B. F. de Chaumont, <i>Netley</i> , | 1876 |
| | Professor Cleland, <i>Glasgow</i> , | 1864 |
| 200 | Dr Cobbold, <i>London</i> , | 1861 |
| | Dr Coghill, <i>Ventnor</i> , | 1870 |
| | Dr P. M. Deas, <i>Macclesfield</i> , | 1868 |
| | Dr J. W. Eastwood, <i>Darlington</i> , | 1871 |
| | Dr R. B. Finlay, <i>Middle Temple, London</i> , | 1864 |
| 205 | Dr Foulis, <i>Cupar-Fife</i> , | 1859 |
| | Dr Fowler, <i>London</i> , | 1847 |
| | Professor Gamgee, <i>Manchester</i> , | 1863 |
| | Peter Gordon, Esq., <i>Juniper Green</i> , | 1861 |
| | Dr A. Graham, R.N., | 1853 |
| 210 | Dr Groesbeck, <i>Cincinnati</i> , | 1875 |
| | Dr Archibald Hall, <i>Montreal</i> , | 1853 |
| | Professor D. J. Hamilton, <i>Aberdeen University</i> , | 1876 |
| | Dr J. H. Hay, <i>Alloa</i> , | 1880 |
| | Dr Stanley Haynes, <i>Malvern</i> , | 1864 |
| 215 | Dr J. S. Howden, <i>Montrose</i> , | 1856 |
| | Dr T. Inglis, <i>Lincoln</i> , | 1878 |
| | Dr James Johnston, <i>Shanghai</i> , | 1871 |
| | Dr George Keith, <i>Currie</i> , | 1845 |
| | Dr Lowe, <i>London</i> , | 1854 |
| 220 | Dr F. R. Macdonald, <i>Inveraray</i> , | 1859 |
| | Dr K. N. Macdonald, <i>Cupar-Fife</i> , | 1880 |
| | Dr John A. Macdougall, <i>Carlisle</i> , | 1875 |
| | Dr John Mackay, <i>Aberfeldy</i> , | 1881 |
| | Professor M'Kendrick, <i>Glasgow</i> , | 1870 |
| 225 | Dr W. O. Mackenzie, D.I.G.H., <i>London</i> , | 1845 |
| | Dr T. J. Maclagan, <i>London</i> , | 1875 |
| | Dr J. Moolman, <i>Cape of Good Hope</i> , | 1877 |
| | Dr J. Ivor Murray, <i>Scarboro'</i> , | 1857 |
| | Dr Andrew Myrtle, <i>Harrogate</i> , | 1853 |
| 230 | Professor Bell Pettigrew, <i>St Andrews</i> , | 1873 |
| | Dr Phillippo, <i>Kingston, Jamaica</i> , | 1860 |
| | Professor W. S. Playfair, <i>London</i> , | 1857 |
| | Dr Priestley, <i>London</i> , | 1854 |
| | Thomas Sheriff, Esq., 1 <i>Chalmers Crescent</i> , | 1867 |
| 235 | Dr Sinclair, <i>London</i> , | 1850 |
| | Dr Francis Skae, <i>Lerwick</i> , | 1864 |
| | Dr T. Skinner, <i>London</i> , | 1856 |
| | Dr John Smith, | 1873 |
| | Dr Van Someren, <i>Redhill, Surrey</i> , | 1845 |

| | | Date of Admission. |
|-----|---|-----------------------|
| 240 | Dr Somerville, <i>Galashiels</i> , | 1877 |
| | Dr Graham Steell, <i>Manchester</i> , | 1877 |
| | Professor Stephenson, <i>Aberdeen</i> , | 1861 |
| | Dr H. R. Storer, <i>Newport, Rhode Island, U.S.</i> , | 1855 |
| | Lawson Tait, Esq., <i>Birmingham</i> , | 1870 |
| 245 | Dr Thin, <i>London</i> , | 1861 |
| | John Traill, Esq., <i>Arbroath</i> , | 1853 |
| | Dr Turnour, <i>Denbigh</i> , | 1843 |
| | Dr T. S. Watson, <i>Bath</i> , | 1852 |
| | Dr W. Watson, <i>Montrose</i> , | 1863 |
| 250 | Dr Walter Weir, <i>London</i> , | 1879 |
| | Dr Yellowlees, <i>Gartnavel Asylum, Glasgow</i> , | 1862 |
| | Professor John Young, <i>Glasgow</i> , | 1859 |

N.B.—Members are requested to communicate with the Secretaries if they discover any errors or omissions in the List, and also to intimate all changes in their addresses.

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TRANSACTIONS
OF
THE MEDICO-CHIRURGICAL SOCIETY
OF EDINBURGH,

FOR SESSION LXIII., 1883-84.

Meeting I.—November 7, 1883.

Dr GEORGE W. BALFOUR, *President, in the Chair.*

I. ELECTION OF OFFICE-BEARERS.

THE following gentlemen were elected office-bearers for session 1883-84:—*President*, Henry D. Littlejohn, M.D., F.R.C.S. Ed.; *Vice-Presidents*, Professor Thomas R. Fraser, M.D., F.R.C.P. Ed.; David Wilson, M.D., F.R.C.S. Ed.; J. Batty Tuke, M.D., F.R.C.P. Ed.; *Treasurer*, Alexander G. Miller, F.R.C.S. Ed., 11 Walker Street; *Secretaries*, C. Watson MacGillivray, M.D., F.R.C.S. Ed., 11 Rutland Street; Alexander James, M.D., F.R.C.P. Ed., 11 Albyn Place; *Editor of Transactions*, William Craig, M.D., F.R.C.S. Ed.; *Members of Council*, P. Heron Watson, M.D., F.R.C.S. Ed.; Byrom Bramwell, M.D., F.R.C.P. Ed.; John B. Buist, M.D., F.R.C.P. Ed.; T. R. Ronaldson, M.B., F.R.C.P. Ed.; George Hunter, M.D., F.R.C.S. Ed., Linlithgow; James Jamieson, M.D., F.R.C.S. Ed.; J. Graham Brown, M.D., F.R.C.P. Ed.; J. M. Cotterill, M.B., F.R.C.S. Ed.

II. ELECTION OF NEW MEMBERS.

The following gentlemen were elected Ordinary Members:—J. Lyon Wilson, L.R.C.P. Ed., Edinburgh, and Donald MacRaild, F.R.C.S. Ed., Greenock.

III. EXHIBITION OF PATIENTS.

1 and 2. *Mr A. G. Miller* showed (a.) A CASE OF PLASTIC OPERATION FOR CONTRACTION OF THE ARM AFTER BURN. The patient

was a little girl, who had been brought into hospital in July, suffering from contraction of the forearm and fixation of the elbow-joint, there being hardly any movement of the arm. The operation adopted was transverse section of the cicatrix. The tendon of the biceps was also severed, when the arm became straight, showing that the elbow-joint was in good condition. To fill up the gap caused by the division of the cicatrix, Mr Miller determined to take a lozenge-shaped flap from the side of the chest. One-half of this flap was accordingly dissected up and attached to the arm, which was fixed to the side of the chest in an almost straight position by means of straps of adhesive plaster. This was done on the 13th of July. On the 23rd the flap was dissected a little further off the side of the chest, to give more relaxation and to make the arm straighter. Later on, during Mr Miller's absence in the country, Dr Maclaren removed the rest of the flap, completing the operation. Two months elapsed between the first and last operations. The vitality of the flap had been proved by an attack of erysipelas, which had occurred after the completion of the operation. The arm was not absolutely straight, but the movement was now very free. (b.) A CASE OF MULTIPLE CONGENITAL TUMOURS OF THE FOREARMS AND LEGS. The patient, an unmarried man, *æt.* 42, had a number of these tumours, more particularly on the left arm. Five were situated along the line of the outer edge of the left ulna, and two near to the left internal condyle. There were two near the olecranon of the right arm, and also one on the front of the right tibia, and two close to the head of the left fibula. The tumours of the arms were quite separable from the skin and mobile over the bone. The tumour in front of the tibia seemed to have some connexion with, or to be adherent to the periosteum, while the two close to the head of the fibula were slightly adherent to the skin. The history was peculiar, patient stating that he had had lumps of the same kind since ever he could remember, but varying in number and size, and never giving pain or inconvenience. On one occasion he pressed some white (sebaceous?) matter out of one of them. When admitted to hospital he was suffering from a pustulo-crustaceous eruption, which had begun to improve. Mr Miller was at a loss to explain the exact nature of these tumours. He was not aware of having ever seen or read of anything like them. They had nothing to do with the skin. They were, every one of them, near to or in the line of some bone, and some of them were apparently moored to the periosteum. The case was not one of molluscum fibrosum, nor did he believe the tumours to be sebaceous, though one had the history of some white matter being squeezed out of it. They were not gummata. Some of them felt like fibromata; others were as hard as cartilage. The greatest difficulty in settling their structure was due to the fact that they had diminished in

size under the administration of potassium iodide (gr. xx. thrice daily). There was also the statement of the patient that the tumours varied in number, size, and firmness, and had always done so since first he noticed them in childhood. The best solution of the difficulty would be to cut one out, but to this the patient objected.

IV. EXHIBITION OF PHARMACEUTICAL SPECIMENS.

Dr W. Craig showed a COLLECTION OF NEW AND RARE DRUGS which had been sent him by Messrs Parke, Davies, & Co. of America. The collection consisted of 28 specimens of drugs of vegetable origin, all of them interesting and important; none of them as yet officinal in our British Pharmacopœia, but several were already officinal in the pharmacopœia of the United States of America, and many of them would undoubtedly become officinal in all pharmacopœias.

Nine of the specimens consisted of leaves of plants:—

1. *Boldo*, whose leaves contain an essential oil, and are used as a nervous stimulant, and are especially useful in chronic affections of the liver. They have been used in this country, more or less, for some years.

2. *Eucalyptus globulus*, the blue gum tree of Australia. These leaves contain an essential oil and are powerfully antiseptic, and the trees are specially useful as a preventive of fever, when planted in malarious districts.

3. *Coca*.—These are the leaves of *Erythroxylon coca*, believed by some to contain thein, and are useful as a nervous stimulant and as an antispasmodic.

4. *Arctostaphylos glauca*.—These leaves are officinal in the United States Pharmacopœia, are used as a tonic and diuretic, and have been recommended for diarrhœa and gonorrhœa.

5. *Yerba santa*, "Holy Herb," the leaves of *Eriodictyon glutinosum*. Its properties are supposed due to a gum-resin, are much used by the natives of Mexico, are good in gonorrhœa, and specially useful in cystitis.

6. *Caroba*, the leaves of *Jacaranda caroba*, a Brazilian plant belonging to the *Bignoniaceæ*, much recommended for syphilis.

7. *Damiana*, the leaves of *Turnera aphrodisiaca*, a native of Mexico, used as a tonic to the generative organs.

8. *Chekan*, the leaves of *Myrtus chekan*, a native of Chili, used as a tonic, expectorant, and diuretic, and said to be specially good in chronic bronchitis.

9. *Lippia mexicana*, a plant belonging to the *Verbenaceæ*, a useful stimulant expectorant, and said to be exceedingly valuable for colds, both in the acute and chronic stages.

Two specimens consisted of flowering tops:—

1. *Grindelia robusta* (wild sunflower), used for asthma, and said to be antagonistic to the poison of rhus.

2. *Grindelia squarrosa* (ague-weed), used for asthma and bronchitis, and said to be specially good in whooping-cough, and, above all, for enlargement of the spleen and for ague.

Seven specimens consisted of flowering herbs:—

1. *Oenothera biennis* (evening primrose), a well-known plant in cultivation, said to be useful in asthma and dyspnoea, and in gastric irritability.

2. *Gentiana quinqueflora* (bilious weed). It has the properties characteristic of the order, an excellent tonic and hepatic stimulant, and is a powerful antiperiodic, and is much used by the Indians for fevers.

3. *Subbatia Elliottii* (quinine flower), a native of Florida, a plant belonging to the *Gentianaceae*, and possessed of tonic and anti-periodic properties, used in fevers.

4. *Frankenia grandiflora* (*Yerba reuma*), possessed of antiseptic properties, used for chronic catarrh and subacute inflammation of the throat, for ophthalmia, for discharges from mucous membranes generally, and for snuffing up the nostrils for fetid discharges from the nasal mucous membrane.

5. *Asclepias currasavica* (blood-flower), so called from its power of arresting bleeding; used for worms; for bleeding it is used both internally and externally, and has been found very beneficial for gonorrhœa and gleet.

6. *Artemesia frigida*.—This has been highly recommended as a febrifuge and as a very good substitute for quinine.

7. *Urechites suberecta* (Jamaica nightshade), a powerful poison. This plant is a powerful depressor of the heart, and has actions not altogether unlike those of aconite. It is not as yet used as a medicine.

There are six specimens of barks:—

1. *Quebracho*, a bark somewhat resembling that of red cinchona, used for diarrhœa, asthma, and general dyspnoea.

2. *Cascara amarga*, a plant belonging to the quassia family, used as a tonic and anti-syphilitic.

3. *Viburnum prunifolium* (black haw), a powerful astringent for loss of blood, and a teaspoonful of the fluid extract every two or three hours is said to be most efficacious in preventing abortion.

4. *Houng-Nan*, the bark of *Strychnos gnatheriana*, a twining plant. Its bark resembles very closely the bark of the *Strychnos nuxvomica*, false angustura bark. It is a native of Southern India, and contains both strychnia and brucia, the latter predominating. It is used in leprosy and for rabies.

5. *Piscidia erythrina* (Jamaica dogwood), a tree belonging to the *Leguminosae*. It is the bark of the root that is used. It has long been used for assisting to catch fish by stupefying them. It is used medicinally as an anodyne and soporific, especially as a substitute for opium.

6. *Rhamnus purshiana* (*Cascara sagrada*, sacred bark), a medicine destined soon to be officinal in all pharmacopœias. It is one of the most efficient remedies for habitual constipation, and the cascara cordial of Parke, Davies, & Co. is at once pleasant to take and a very valuable medicine for habitual constipation.

Of roots there are four specimens:—

1. *Berberis aquifolium*, said to be a very powerful tonic.

2. *Franciscea uniflora* (manaca), a plant belonging to the *Scrophulariaceæ*, a native of Brazil, sometimes called *Mercurio-vegetal*. It is said to stimulate the lymphatics, and so eliminates morbid products from the system. It is used for rheumatism.

3. *Piper methysticum* (kava-kava), used in the Sandwich Islands for producing an intoxicating drink. It contains an acrid resin and a volatile oil. It has been recommended for gonorrhœa, gout, etc.

4. *Sarracenia flava*, one of the pitcher-plants, is a very powerful astringent, and specially good in diarrhœa.

These specimens are beautifully prepared, excellently labelled, and it is hoped that their virtues will be extensively experienced also on this side of the Atlantic.

V. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Dr W. Craig* showed a pathological specimen illustrating the rare malformation of the RECTUM ENDING IN THE MEMBRANOUS PORTION OF THE URETHRA, which will form the subject of a future communication to the Society.

2 and 3. *Dr C. W. MacGillivray* showed (a.) THREE SPECIMENS OF CALCULI. The first of these was a uric acid stone, coated with phosphates, from the bladder of an old man. The specimen was interesting from the fact that the patient had been unsuccessfully sounded several times with a short-beaked sound. It was discovered accidentally one day on the introduction of a catheter with a wide curve. The second specimen was a series of small uric acid calculi with facets, from the prostate of an old man who having stricture, had suffered twice from extravasation of urine. They were removed by the median operation. The third was a uric acid calculus from the prostate of a man who had had trouble with his urine ever since he had suffered an injury to his perineum when fifteen years of age. When admitted to hospital he was suffering from perineal abscess. When this was opened, a quantity of fetid pus escaped. The finger, passed into the abscess cavity, found the calculus fixed in the prostate. A gush of water followed on its removal. The perineal abscess had unfortunately burst the pelvic fascia before it was laid open, and the patient died. (b.) A DEGENERATED OVARY AND FALLOPIAN TUBE, REMOVED FROM AN INGUINAL HERNIA. The patient was a girl about 20, who suffered from a swelling in the left inguinal region about the size

of a small orange. Becoming larger it interfered with her going about. The diagnosis was either a cystic disease of the round ligament, or hernia of ovary or Fallopian tube. As it could not be reduced it was cut down upon, and was found to contain a degenerated ovary and Fallopian tube. They were removed and the wound closed. The girl made a good recovery. There was no disturbance of menstruation before or since the operation.

4. *Dr A. Balfour* of Portobello showed a HALFPENNY that had lain in a boy's stomach from 1st January to the 28th October of the present year. There were no symptoms of its presence from the time it was swallowed till it was ejected during vomiting.

5. *Dr P. H. Watson* showed three specimens of CALCULI. (a.) A uric acid calculus from the bladder of a gentleman *æt.* 75, on whom he had performed lithotripsy some ten years previously. On this occasion the patient suffered from such a degree of inflammatory action in the mucous membrane of the bladder that lithotripsy must have proved fatal. Lateral lithotomy was performed, and the patient made a good recovery. (b.) An oxalate of lime calculus, coated with crystals of triple phosphate. This patient was aged fifty years. His history appeared to be one of stone from an early period of life. He had been examined by many surgeons, but all had previously failed to discover the existence of the calculus. It was removed by the lateral operation. (c.) Fourteen calculi, also removed by the lateral operation from the bladder of a patient *æt.* 78, who had been under the care of several specialists in London. He came to *Dr Watson* for stricture, but the inflammatory reaction was more than could be accounted for by the stricture. The sound showed that there was at least one large calculus, but when the operation was performed fourteen were removed.

6. *The President* exhibited a PHOTOGRAPH OF A LIGHTNING PRINT from the arm of a boy who had been struck by lightning near Duns on the 16th of June last. He also showed the terminal branch of an isolated yew tree, which grew about 70 yards from where the boys were, and had been struck at the same time. Several large yew trees, also growing in the neighbourhood, had much of their foliage scorched; no other plant was injured. Such lightning prints were well known, and had often been taken to be impressions of shrubs or trees growing near those struck. In this instance the yew branch exhibited had been supposed to be the object represented. A writer in the *Photographic News* of 6th July had rightly enough stated that it cannot be a photograph, as it is a *positive* or *dark* picture on a light ground. While, if it had been formed according to the chemical and optical laws essential to the production of the photographic shadow of a tree, the picture would have been negative, that is, *light* upon a *dark* ground. In all probability the markings developed on the arms and necks of the boys

were due to the direct action of the electric fluid paralyzing the vessels and causing temporary congestion of the capillary vessels. This was the first known instance of a photograph taken from a lightning print. The Society had to thank Dr Watson Campbell of Duns for this opportunity of seeing it, as well as for the following most interesting account of the accident:—"A violent thunderstorm passed over Duns on the 16th of June last, reaching its climax about 11 A.M. Just before that hour four boys took shelter in a stable at Cairnbank, half a mile from the town. A very vivid flash, followed instantaneously by a terrific peal of thunder, startled the neighbourhood. Immediately the gardener heard some one in the stable calling out "Murder! murder!" and on going thither he found two boys quite unconscious (one apparently dead), and other two stupid and alarmed. One of them was calling "Murder;" the other had been driven through the open door across the road, and was in the act of crawling back. They were taken to the gardener's house, where the worst case vomited, and all soon became conscious. One said he could not feel his legs; another said his arms were cut off; and all were found, when



taken home and undressed, to be strangely marked, and a burning feeling remained in these marks after all other peculiar sensations had gone. The marks were of a bright pink colour, and looked

like accurate representations of some small-leaved plant or shrub. I saw two of the boys; there was one mark over the ball of the right shoulder exactly like a terminal twig; on the left arm, from shoulder to elbow, there was a markedly arborescent mark (*vide* woodcut from photograph taken four and a half hours after the accident).¹ A similar mark was found on the other boy, extending down the inside of the right arm from axilla to shoulder joint, and for five or six inches down the side of the body. The mark entirely disappeared during the night. Only one of the boys said he saw "a ball of blue fire" before becoming unconscious. Two say they smelt sulphur when returning to consciousness. None of their clothing was singed even in the slightest degree."

VI. VALEDICTORY ADDRESS.

By the retiring President, GEORGE W. BALFOUR, M.D., Pres. R.C.P. Ed., Consulting Physician to the Royal Infirmary and to the Royal Hospital for Sick Children, Edinburgh.

FOR as far back as we have any records of the proceedings of this Society, it has been customary for the retiring President to give a general *résumé* of the work done in the Society during his two years' occupancy of the chair. In a society like the present, which has always comprised among its members some of the most advanced thinkers and most able teachers of medical science, this biennial retrospect has had this manifest advantage, that from one fixed period to another it indicated the lines in which medical thought was progressing, as well as the nature and extent of the progress made. This time-honoured custom has not, however, been always adhered to. It has been varied from time to time by original essays of great value, one of the earliest of these being by Dr John Gairdner "On the Importance of Medical Statistics," another by Dr Sellar "On Medicine as an Art, and the Nature of its Scientific Basis," and a third by Sir James Simpson "On the General Progress of Medicine." At the present time it seems less needful than usual to summarize the work of the two sessions during which I have had the honour to preside over you, inasmuch as the Society has now returned to its original practice of publishing its Transactions, and each of you possesses, in two handsome volumes, the complete record of all the work that has been brought before us during these two sessions, as well as of the discussions to which that work has given rise. I have therefore thought that it might be more interesting to you, and probably quite as instructive, if I were to give you an account of the progress made, not during the last two sessions only, but during all the period with which I have been connected with the Society. But as the progress in medical science during that period has been so great, that it would be im-

¹ We have to acknowledge the courtesy of the Editor of the *Lancet* in favouring us with the use of this woodcut.

possible to review the whole of it in the limited time at my disposal, I shall restrict myself to one or two subjects only, pointing out the influence of this Society in developing the opinions presently held regarding them, and their important bearing on the evolution of medical science, as well as on the wellbeing of mankind.

The first subject I shall take up is OVARIOTOMY, an operation which has attained a great success in our day, but one which was for the first time discussed in this Society just a year before I joined it. You are aware that the removal of the healthy ovaries from the females of the lower animals, to prevent breeding and secure an early maturity for the purposes of the butcher, has been an operation carried out from time immemorial with but little danger to life. Some of the nations in olden times, notably the Arabians and Lydians, were also in the habit of making what were called female eunuchs. But the removal of diseased ovaries for the purpose of relieving suffering and prolonging life is altogether an operation of modern development. It is little more than one hundred years since L'Aumonier,¹ the chief surgeon of the great hospital at Rouen, removed a diseased ovary for the first time, I believe, in medical history. Single cases, more or less successful, followed from time to time in France, Germany, and America. But little progress was made, however, till Dr Macdowal of Kentucky recorded three successful cases, the first of which occurred in 1809, and the last in 1816. In 1823 the first operation of this kind in Edinburgh was performed by the late Mr John Lizars. And in this remarkable case the whole of the profession in Edinburgh were satisfied that one ovary was diseased, while all but Dr Campbell and Mr Lizars were agreed that the operation was unjustifiable. Nevertheless, when the operation was completed, both ovaries were found to be perfectly healthy, and the woman made an excellent recovery. Surely a most remarkable instance of the fallibility of medical opinion. Encouraged by this qualified success, Mr Lizars subsequently operated upon three other patients, of whom one died, while in the other two the operation was not completed. In one of these both ovaries were diseased, and only one could be removed; in the other the diseased ovary could not be removed on account of its extensive adhesions.² From this time onward ovariectomy continued to be occasionally performed, but with so little success, both as regards the accurate diagnosis of the case and the ultimate result to the patient, that the late Dr Lee of London has stated that "on the practice of extirpating the ovaria when diseased, it is not necessary to offer any observations, as it has been abandoned by all who have made themselves acquainted with the pathology of these organs."³ This statement was made in 1834, and so late as January 1844, just two months

¹ *Observations on the Extraction of Diseased Ovaria*, by John Lizars, Surgeon, Edinburgh, 1825, p. 3.

² *Op. cit.*

³ *Cyclopedia of Practical Medicine*, vol. iii. p. 231, 1834.

before Dr Atlee of Philadelphia commenced what was to prove his most successful career as an ovariologist, a reviewer on this side of the Border gave utterance to very similar sentiments¹ in noticing Mr Clay's, of Manchester, first series of cases.² Shortly after this ovariotomy was for the first time brought under the consideration of this Society by the late Professor Bennett, who read a paper entitled "Observations on Ovarian Dropsy."³ Only a very imperfect abstract of this paper has been published, but it apparently contained an original account of the pathology of ovarian cysts, a claim that the diagnosis might be greatly facilitated by a microscopic examination of the fluid removed by tapping, and by the use of Sir James, then Dr, Simpson's uterine sound; it also contained a full report of a case in which, at the author's request, the late Dr Handyside had performed ovariotomy, the patient dying on the 70th day. This paper was considered so important that, at Dr Cormack's request, a special night was set aside for its discussion, which is also fully reported in the journal he at that time edited.⁴ In commencing the discussion, which I need not detail in full, Sir John Rose, then only Dr Cormack, stated that up to May 1845, 89 cases of ovariotomy had been recorded, in 14 of which the operation had been abandoned after the woman was opened, either because there was no tumour or because its attachments could not be meddled with. And Dr C. claimed that all the great surgeons of the day rightly denounced ovariotomy as—"An operation in almost every case unwarrantable—1, because of almost insuperable difficulties attending the diagnosis; 2, because ovarian tumours are not fatal; 3, because of the immense and inevitable dangers of the reparative process."⁵ Dr Handyside defended the operation, contending that, though generally inadmissible, "yet in such rare cases as the present, *but in such only*, he was quite prepared to repeat the operation."⁶ Dr Handyside, as some of you may remember, was always willing to adopt anything that looked like a rational improvement in surgery, and was one of the earliest to intrust the vessels in an amputation through the thigh to acupressure.⁷ Sir James Simpson followed him with one of his ablest and raciest speeches, which even yet it is a treat to read. He acknowledged that ovariotomy was a most difficult and dangerous operation, but he regarded it as not only justifiable, but urgently required in certain cases for which medicine had neither remedy nor relief. He looked forward confidently to the time when this operation would be recognised as a legitimate operation, when, by improvements in the method of diagnosis, fitting cases would be more readily selected, and, by improvements in the

¹ *Monthly Medical Journal*, 1844, p. 67.

² *Cases of Peritoneal Section for the Extirpation of Discused Ovaria*, by Henry Clay, M.R.C.P.E., London, 1842.

³ *Monthly Medical Journal*, Jan. 1846, p. 53.

⁴ *Loc. cit.*

⁵ *Loc. cit.*, p. 54.

⁶ *Loc. cit.*, p. 56.

⁷ *Edinburgh Medical Journal*, Dec. 1860, p. 504.

methods of operating, the mortality would be greatly lessened, as had already happened in other fields of surgery. "How different," he says, "is amputation now from what it was formerly, with the hot iron, or boiling pitch, to seal up the cut vessels. How comparatively safe and simple is the tying of an artery now from what it was half a century ago, with its flat double ligatures, and ligatures of reserve, etc." He further twitted the surgeons with denouncing ovariectomy as unjustifiable, mainly because, though they operated, they were unable to diagnosticate the tumours, and required an obstetrician to aid them; while the foremost surgical authorities still regarded ligature of the innominate as a justifiable operation, though it had proved fatal in every case, instead of only in one out of every two or three, like ovariectomy. He further supported his position by an appeal to the statistics of the major operations as performed in Paris, Glasgow, and Edinburgh; concluding his most eloquent speech by some novel suggestions as to the performance of the operation, notably one first made to himself by Dr George Keith, to pass the pedicle out into the vagina, thus both providing free drainage as well as enabling the abdominal wound to be more securely closed. This is noteworthy as the first connexion of the family name of Keith with an operation which was destined ere long to make it so famous. I need not say that Sir James's charming way of making things rather hot for his opponents, in this case the surgeons, brought a hornets' nest about his ears, in the shape of recriminative letters showered upon him through the journals for the next month or two. But Sir James could always hold his own, and at this meeting of the Society he certainly had the best of it. Mr Spence, who had refused to operate in the case under discussion, defended his position, and denied that ligature of the innominate was a justifiable operation. But Sir James, quoting in reply from Mr Fergusson's and Mr Syme's text-books, showed that he had right on his side, and adduced some further statistics supporting his view of the subject. The late Dr Spittal, speaking in support of the operation, pointed out statistically that errors in diagnosis had been even more frequently made in connexion with aneurisms than with ovarian tumours, and he regarded ovariectomy as quite as justifiable from every point of view as ligature of the larger arteries for aneurism. But he was only Physician.

In April 1848¹ Sir James Simpson returned to the charge, bringing with him Mr Clay of Manchester, who had that morning performed ovariectomy for the 28th time, and who naturally spoke in favour of it, confessing, however, that it had not been successful in Edinburgh. Simpson again based his support of ovariectomy upon comparative statistics, but stated that he would select his cases, and would not operate except in cases otherwise likely to prove speedily fatal. Dr Bennett, who had

¹ *Monthly Medical Journal*, April 1848, p. 761.

been absent from illness from the former discussion, defended the operation for pathological reasons, and said that his case had died after 70 days, not from the operation, but from ileus following a surfeit. Three years subsequently, ovariectomy is again brought before this Society by a report by Dr Myrtle of the death and dissection of Magdalene Berry, twenty-five years subsequent to Mr Lizars's unsuccessful attempt to remove her tumour, which was found very much as he had left it. Naturally enough the president, Mr Syme, took advantage of this case to urge anew the unjustifiable character of the operation of ovariectomy. Simpson, however, again came to the front in defence of this operation in certain cases.¹ Five years afterwards ovariectomy came before this Society on the report of an unsuccessful case operated on by Mr Edwards,² and this was a great field-night. The discussion was led off by Dr Matthews Duncan, who, in a long and able speech, contended that ovarian tumours in all cases likely to be selected for operation were not dangerous to life, "were quite consistent with a moderate, and occasionally with a long, continuance of life;" he also said that to justify this operation it had been argued that tapping was a dangerous operation, was "fatal in about one in every five cases operated on." But, as every one knew, tapping was an operation daily performed with but little risk: he himself had tapped one woman forty-six times. He disapproved entirely of the manner in which statistics had been employed to justify this operation, and he believed that, were proper statistics adduced, ovariectomy would be found "far to exceed the mortality of almost all recognised surgical operations for even the most hopeless diseases. . . . The farther we cautiously advanced in this difficult question, the more we were forced to resort to the opinions of great and wise surgeons,—and they condemned the operation." Sir James Simpson again vigorously fought for the legitimacy of the operation. He pointed out that Mr Southam had published a limited table of first tapplings, in which the mortality was one in five, and he begged Dr Duncan to recollect that it was first tapplings only which were serious, and that comparatively little risk attached to the repetition of the tapping. Ovariectomy was undoubtedly a very dangerous operation, but not more so than many surgical operations for chronic disease. Amputation at the hip-joint, for instance, quite a justifiable operation in the eyes of most surgeons, was fatal in 70 out of every 100 cases; ovariectomy, an operation not justifiable in the eyes of the same surgeons, was only fatal in 40 out of every 100 cases. Dr Clay of Manchester had recently published 71 cases of ovariectomy, of whom only 22 had died, little over 30 per cent. While even in these cases the mortality

¹ *Monthly Medical Journal*, Feb. 1851, p. 198.

² *Edin. Medical Journal*, April 1857, p. 894. Discussion on this case will be found in the *Ed. Med. Journal*, Feb. 1857, p. 752.

had been a gradually decreasing one, 8 having died out of the first 20 = 1 in $2\frac{1}{2}$, 6 out of the second 20 = 1 in $3\frac{1}{2}$, and only 8 out of the last 31 = 1 in 4. Dr Duncan objected to ovariectomy because "surgical instinct" was against it. But surgical instinct was a misleading quality; it had formerly led the best surgeons to trepan in cases in which later experience had shown that such an operation was unnecessary. Surgical instinct had also led some of our greatest surgeons to tie the innominate for the carotid aneurism, yet of fourteen or fifteen cases all had proved fatal. The one great and powerful reason why ovariectomy had not been taken up by British surgeons, was because they were so ignorant of female diseases that they could not make a proper diagnosis without the aid of an obstetrician. Some foreign surgeons had devoted themselves to the study of female diseases, and in time this difficulty would be got over. He believed ovariectomy to be principally, if not solely, justifiable in cases where an otherwise healthy woman was threatened with speedy death from the progress of the disease, and where other treatment had either failed or seemed inadmissible for various reasons. Doubtless ovariectomy might be much more successful if undertaken at an earlier period of the disease, before the tumour had become so large or so active as to threaten life. He himself, however, doubted the propriety of undertaking this and other formidable surgical operations except for some very clamant reason. Those, however, who thought otherwise were supported in their views by those surgeons who habitually urged early operation in cases of stone, of aneurism, external tumours, etc. If the principle of early interference in such cases were admitted, it would be difficult to prove that it did not in all points and with equal force apply to ovarian cases. He could not help doubting whether in this and in some other respects the whole code of surgical ethics did not require revisal. Dr Keiller saw no reason to regard ovariectomy as an unjustifiable operation: he would have no hesitation in performing it should a suitable case come across him. Dr Moir agreed as to the danger of tapping, and stated that many patients succumbed to the first performance of this apparently trivial operation. He believed in the success of the operation in Dr Clay's hands, but did not seem to have any urgent desire to perform it himself. In answer to Dr W. T. Gairdner, it was elicited from Dr Black (late of Manchester) that Dr Clay had of late been more careful in the selection of his cases, and that his later operations had been on tumours of a smaller size. The President (the late Professor Miller) congratulated the Society on both sides being agreed that the cases suitable for the operation were most rare. "He could conceive of such cases. They demanded the following postulates:—1st, No hope from any other treatment, and death inevitable at an early period otherwise; 2nd, Reasonable expectation of success in all the circumstances; 3rd, An accurate diagnosis

as to existence of ovarian disease, as to its being more or less solid in structure, as to its non-adhesion to the parietes, and as to its being attached by a narrow peduncle ; 4th, The patient intelligently aware of the nature of her case and of the risk of the operation, and, notwithstanding, anxiously and urgently resolved on its performance. The difficulty obviously lay in the third postulate. How is such accuracy of diagnosis to be obtained? He could conceive of such cases, but he had never met with any, and he hoped he never would; for he was bound to confess that his instincts were all against such operations, and it was a positive relief to be assured by Dr Keiller that he would be specially exempted from all responsibility in relation to cases coming under that gentleman's care." He called it not *abdominal*, but *abominable* surgery; and though he had no doubt that Mr Edwards had performed the operation admirably, yet he was sure that if he and Mr Edwards were spared to grow old together, the result of their combined retrospect would be to range "surgical experience" more disapprovingly of such operations than perhaps "surgical instinct" might incline to do now. Mr Edwards replied to Mr Black by reading an extract from Dr Clay's work, in which he stated that at first he was more afraid of adhesions than latterly, and that he had formerly rejected many cases on which he would not hesitate then (*Handbook of Obstetric Surgery*, 1856, p. 162). Mr Edwards also remarked that "the opinion on this or any other operation by men now eminent could not be considered final, as operative surgery was a progressive art." The magnitude of the operation should not make the surgeon overlook the smallest details, for on them mainly success depended; and a surgeon should set about removing a tumour of forty pounds weight with as great attention to the various steps of the operation as he would employ in operating for cataract, or in making an artificial pupil. These wise and prophetic remarks conclude the first chapter in the history of ovariectomy in connexion with this operation. A new era was about to arise, a more successful operator was in preparation. On the 15th of July 1863, Dr Thomas Keith read to this Society¹ an account of six cases in which he had performed ovariectomy, four of whom had recovered. These were the first successful cases in Edinburgh since Mr Lizars's solitary case in 1823; they were not selected cases, or, if so, were selected for their severity. One of these tumours was, I suppose, the largest ever removed—it weighed 120 pounds, being heavier than the woman herself. She had previously been a patient of my own, and arrangements had at one time been made for the removal of the tumour by Sir James Simpson, but at the last moment he refused to operate. The operation was undertaken by Dr Keith a year or

¹ *Ed. Med. Journ.*, Oct. 1863, p. 369. Keith's paper will be found *in extenso*, *loc. cit.*, p. 299.

two subsequently. I was asked to be present, but refused, as I did not believe so large a tumour could be removed without the patient dying on the table. She herself told me that towards the close of the operation she heard one of the assistants say, "She is dead;" but she added, "I said to myself, I am not dead, and I don't mean to die if I can help it." I have reason to know that this statement is true, and that the operator, true to his character, instead of hastily closing the wound as he was urged to do, finished the operation with as much care and deliberation as if the woman were actually alive, and was rewarded by finding her to be so. She lived for nine years, and for a time was naturally an object of great professional interest. But one day a French surgeon, who had been duly informed of the immense size of the tumour and the difficulty of the operation, glanced first at the cicatrix, now contracted to a few inches and lying transversely, and then at Dr Keith so expressively, that thereafter I believe no surgeon was asked to see her. In his paper Dr Keith remarked, "There is no operation that has had so much evil spoken of it as ovariectomy. No surgical proceeding ever brought forward for the saving of life has been so unfairly treated; not one has had to pass through such an ordeal of opposition, ridicule, and unbelief, and none has come so triumphantly out of it." And again, "It is a proceeding applicable to the majority of cases of multilocular disease, and in a short time the simple rule of treatment will, I believe, be *to wait till the life of the patient is threatened by the disease*, and then, in suitable cases, to recommend the radical cure; and every successful case will be a life saved."

Mr Spence quite agreed with Dr Keith, that we should wait till life was in danger; then the operation was not only warrantable but imperative. It was scarcely fair to say that ovariectomy had been unfairly treated by surgeons; at first the diagnosis was very uncertain and the results most unsatisfactory. Nowadays the operation was on a different footing, and must be regarded as perfectly justifiable, if not performed too early. Sir James Simpson, after congratulating Dr Keith on his well-deserved success, said he did not agree with Mr Spence that ovariectomy had not been unfairly treated by surgeons. "No doubt the diagnosis had been much improved of late years; but surgeons were not justified in having opposed ovariectomy, as they had so bitterly and constantly done, on the ground that the diagnosis of ovarian disease was not perfect, for this consideration did not deter them from operating in other cases where their diagnosis was very imperfect also." It was a grave mistake to regard ovarian diseases as "unsightly rather than dangerous." When left to itself, multilocular disease of the ovary was a very fatal malady. Mr Spence and Dr Keith agreed in thinking it unjustifiable to operate unless there was danger to life. To this doctrine Simpson demurred. If we waited

till life was endangered, in many cases we would wait too long; the operation would have no chance of success. Surgeons did not generally act on this principle. When they found a stone in the bladder an immediate operation was recommended to avoid increased suffering and danger, which were sure to follow delay. Formerly it was thought ovariectomy should not be performed when there were adhesions; these are not now regarded as obstacles to the operation. Ovariectomy was now quite as successful as many of the formidable operations in surgery. Some had said that ovariectomy patients should only be operated on in the country, and never in a hospital. If this was true as to ovariectomy, it would be equally true of all surgical operations. Spencer Wells and Baker Brown had not acted on this principle, and their success was well known. Simpson concluded by remarking that one of the most important questions to be solved was the treatment of the pedicle and the mode of applying the clamp.

Dr Moir agreed as to the fatality of ovarian tumours, and alluded to the disastrous results of firstappings. Sir James Simpson had long ago pointed out the risk of first tapping. Of 130 cases of firstappings by Lee, Southam, Kivisch, and others, 30 had died before a month had elapsed. With larger data the mortality might be found to be actually less, but the operation was unquestionably often a fatal one. Dr Matthews Duncan did not think that ovariectomy had been unfairly treated by surgeons, for, as soon as success had attended the operation, its position had been established. Dr Duncan "could scarcely think that attention to minutiae could have great effect in determining the good or bad results of the operation. . . . No doubt, everything should be attended to, but it scarcely seemed to him that any little change in the mode of applying the clamp could make much difference." He did not agree with Simpson as to the danger of tapping; he had never seen any one die or suffer from this operation. Dr Keiller congratulated Dr Keith on his success; he had frequently seen him operate, and greatly admired the able manner in which difficulties had been met and overcome. There could be no doubt that in Dr Keith's hands this operation would prove yet more successful. Meanwhile his paper had accomplished much to be thankful for; it had compelled this Society to acknowledge the justifiable character of this operation.

And now I take leave of this operation; from this time onwards its history is an almost unbroken succession of triumphs, a perfect *currus triumphalis*. Four successful operations had done more to open the eyes of the profession to the legitimate character of the operation than forty years of argument. Ovariectomy had fallen into the hands of an able man, and, as Dr Keiller ventured to prophesy, his success has gone on increasing. And the success is well deserved. When a man can say that of close upon 500 cases of abdominal section he has only been wrong in his

diagnosis four times, his *tactus cruditus* must be as nearly perfect as anything sublunary can be. How carefully he had trained himself for the career to which he has so greatly devoted himself may be learned from the fact, that so lately as nineteen years ago it was argued in a series of letters in the *Lancet* (1864) that an absolutely accurate diagnosis could not be made without an exploratory incision, yet Dr Keith had operated 136 times without ever making an error in his diagnosis. This we have on the authority of Dr Peaslee of New York, himself an accomplished ovariologist, who says that Dr Keith's success—the highest in Europe—has been gained, not by a careful selection of cases, for many of them were most unpromising, but by his “great accuracy as a diagnostician, his extreme delicacy and cautiousness as an operator, and, not least, by his most conscientious and unremitting care during the after-treatment.”¹ Dr Keith's latest publication² has proved that ovariectomy is not an operation requiring a special locality or a country atmosphere, but is one which may be successfully carried out even in the wards of an ordinary hospital. Of 79 cases operated on in the Edinburgh Infirmary, 69 ovariectomies and 10 hysterectomies, only 5 have died. Of all the cases of abdominal section, 82 in number, including the foregoing, 1 case of interstitial pregnancy, and 2 cases of removal of the ovaries and tubes, 6 have died, little over 7 per cent.; while, as if to show that this success is due to the operator himself and not to any of our modern methods, of 24 cases treated with carbolic acid spray, 4 died = 16·6 per cent., and, rightly or wrongly, some of the deaths were attributed to the carbolic acid, which, moreover, did not always prevent septicæmia; and of 2 cases treated with the boro-glyceride spray, 2 died; while of the last 56 treated without spray at all, only 1 died = 1·78 per cent. Forty years ago who could have anticipated so brilliant a success for an operation then denounced as unjustifiable—little short of murder—by every surgeon in Britain? And while we do honour to him by whom this great success has been achieved, we ought not to forget those pioneers to whom we owe much—more, perhaps, than at first we are willing to acknowledge—John Lizars, the successful operator, forced to give up the operation because the diagnosis of abdominal tumours was not far enough advanced to enable him to know whether a tumour was present or not; John Hughes Bennett, who first gave a scientific reason for operating, by showing that the pathological nature of ovarian tumours tended to ever-increasing growth till the organism succumbed, and who also first introduced the microscopic examination of the fluids as an aid to diagnosis; and, last, not least, Sir James Young Simpson, who, with his vigorous intellect and indomitable

¹ *Ovarian Tumours, their Pathology, Diagnosis, and Treatment, especially by Ovariectomy.* London, 1873, p. 329.

² *Report on Cases Treated in Ward XIX. of the Royal Infirmary, Edinburgh.* Edinburgh, 1883.

energy, fought the battle of ovariectomy, till he had the satisfaction of seeing it occupy the position he had always ascribed to it, of a justifiable, because a life-saving, operation, and, himself an unsuccessful operator, had the pleasure of cordially congratulating his younger rival on his unprecedented success.

The next subject of which I shall speak to you is one which has involved a change of practice among physicians even more remarkable than that we have just seen has taken place among surgeons in regard to ovariectomy. It is a change, moreover, involving much wider interests, inasmuch as it concerns every one more or less, and not merely a few women. As I myself was the first to broach the subject of this change in this Society, I do not in this case require to travel beyond my own immediate connexion with it; but in order to give you some idea of the greatness of the change, I shall first of all give you a short preliminary sketch of the state of affairs which led up to my first paper. The lancet, in those days, was in every one's hand—like Fitz-James's blade, it was both "sword and shield;" scarcely a disease which it could not cut short or ward off, according as it was employed after or before its onset. In every almanac were given the proper days and proper seasons, mainly spring and autumn, for letting blood periodically to maintain the body in health and vigour. Physicians prescribed it liberally to cure inflammations; surgeons employed it freely to ward off inflammations. An old gentleman once told me that, having been accidentally stabbed in a scuffle in Leith Street, he made his way to the door in St Andrew Square of the great Edinburgh surgeon of those days, the grandfather of our worthy late treasurer, and having rung his bell, he fell fainting on the doorstep from loss of blood. When he awoke to consciousness he found himself again bleeding, but this time from a vein opened, *secundum artem*, to prevent inflammation. The late Dr Stokes of Dublin says, "When I was a student of the old Meath Hospital, there was hardly a morning that some twenty or thirty sufferers from acute local disease were not phlebotomized. The floor was running with blood; it was dangerous to cross the prescribing hall for fear of slipping."¹ Even fevers did not escape. Not only were the apparently acute symptoms of relapsing fever met by bloodletting, but "the cerebral symptoms of typhus fever were met by opening the temporal artery, or by a large application of leeches to the head."² Marshall Hall, who knew better than most men the risks and dangers of bloodletting,³ found it indispensable as a means of diagnosis, and bled patients to syncope to find out whether apoplectic symptoms were due to *ramollissement* or not, or whether a

¹ *Lectures on Fever*. London, 1874, p. 17.

² Stokes, *loc. cit.*

³ *Researches Principally Relative to the Morbid and Curative Effects of Loss of Blood*. London, 1830.

patient had peritonitis.¹ Is it possible to conceive a state of matters more diametrically opposed to what now exists? In the midst of all this—while Marshall Hall still taught, in spite of full knowledge of its risks, that bloodletting was “the remedy—the only remedy—for inflammation;”² and Alison, that in the treatment of pneumonia, “uncomplicated and recognised from its commencement, the utmost confidence may be placed in general bloodletting, which should always be large”³—a thunderbolt fell among the profession. It was claimed by men of undeniable ability and professional skill, that the administration of a few decillionths of a grain of phosphorus was quite as efficacious in the treatment of pneumonia as the abstraction of many ounces of blood. A statement so remarkable as this could not be allowed to pass unchallenged; it was absolutely disbelieved, and there was scarcely a physician in Britain⁴ who was not prepared to deny that patients recovering under such a treatment could have had pneumonia. It was true, nevertheless. I satisfied myself of that during a nine months’ residence in Vienna, with an almost daily visit to Fleischmann’s Hospital;⁵ and I was perhaps only not converted to homœopathy because my equally daily visits to the General Hospital had taught me that the heroic virtues of infinitesimal doses of phosphorus were fully equalled by those of *extractum graminis*—hay tea, and that, as Skoda taught, pneumonia was not a disease tending to dissolution, but to resolution.⁶ On my return home I was admitted a Member of this Society, and took an early opportunity of bringing before it a notice of the “Practice of Skoda,” which had proved so interesting and instructive to myself, giving an account, among other matters, of 392 cases of pneumonia treated with *extractum graminis*, or some equally innocuous remedy, with a mortality of only 54, 13·7 per cent., or nearly 1 in 7, while the mortality under more heroic treatment in the Edinburgh Infirmary, for the five years previous to September 1844, had been over 35 per cent., or nearly 1 in 2. I concluded by stating that some of the best physicians in Vienna had told me that disease had in their day undergone a gradual change in type, and was less amenable than formerly to heroic treatment; that a similar change appeared to be taking place in Britain, and that it would be wise to modify our practice in conformity with this change

¹ *Monthly Journal*, Sept. 1843, p. 764.

² Marshall Hall, *Researches*, etc., p. 89.

³ *Outlines of Pathology*. Edinburgh, 1844, p. 280.

⁴ The late Sir John Forbes, editor of the *British and Foreign Medical Review*, was an exception, and his paper on “Nature and Art in the Cure of Disease” was the earliest indication of the turn of the tide.

⁵ Vide *British and Foreign Medical Review*, Oct. 1846; and *Introduction to the Study of Medicine*, Edinburgh, 1865, p. 253.

⁶ My first impressions of Skoda’s treatment will be found in the *Northern Journal of Medicine*, 1846, p. 55.

in the constitution of disease.¹ The late Dr Gairdner, in reply, acknowledged that he believed in a change in the character of disease, and that acute pneumonia, for instance, formerly required much larger bloodlettings than now. He thought "nothing was better established than the good effects of bloodletting in the acute pneumonia of Edinburgh, whatever might be the case in Vienna. . . . Of the benefits of *early* bloodletting he entertained no doubt whatever; they were positive, immediate, unequivocal, and admitted by almost every physician whose experience and judgment entitled him to consideration; and if Dr Balfour or any one else could shake his conviction in the truth of this opinion, he would also succeed in producing in his mind a general distrust of medical evidence in all cases of every description, since in no case whatever can we have evidence which is stronger or more satisfactory." Dr Bennett fully agreed with the remarks of Dr Gairdner. He attributed much of the change which had latterly occurred in practice to an improved pathology and diagnosis, whereby the nature of diseases was better understood, and their detection rendered more exact. "Dr Balfour had attempted to establish the benefits of a 'do-nothing practice' from the results of statistics. Medical statistics, as he would endeavour to show at the next meeting, were altogether fallacious, and undeserving the slightest confidence." Dr Bennett concluded by strongly condemning the system of practice lately sought to be introduced by Dr Forbes and others, to the exclusion of pathology, diagnosis, and the experience of the most eminent men. So the matter rested for a time; but the profession were on the horns of a dilemma, for the mysterious decillionth of a grain of phosphorus was unquestionably as efficacious as phlebotomy. The history of medicine might have rescued the profession from its difficulty, but it seemed to be unknown. Fortunately the meaning of *extractum graminis* was not abstruse, and in time had its effect. In 1855 Dr Bennett published an introductory lecture, in which he acknowledged that large bleedings in the treatment of pneumonia had been abandoned, and that the mortality had greatly diminished under this expectant treatment, not because there had been any change of type, but because large bleedings were never really required, and had also been attended by dangers which had neither been understood nor even recognised.² This at once brought the late Dr Alison to the front, and inaugurated the great change of type controversy,

¹ "Notes on the Practice of Skoda," read June 1847; published in the *Edin. Med. and Surg. Journal*, 1847, p. 397. The discussion upon it will be found in the *Monthly Med. Journal*, Aug. 1847, p. 142. In 1857—*vide Ed. Med. Journal*, p. 403—Dr Arthur Mitchell says that in Vienna he found no physician attributing the change in treatment to a change of type, but that all regarded it as due to more careful observation of the effects of treatment. This view also prevailed among the younger men in 1847. The opinion I have given was that prevalent among the older men at that date.

² *The Present State of the Theory and Practice of Medicine*, Edinburgh, 1855.

which can scarcely be said to be settled even yet, though the indications of a recurrence to olden views are getting more and more rare; and the pathology of pneumonia is now trending off in a direction which will necessitate the battle being fought on another field if the fight is renewed at all. Dr Alison¹ had no hesitation in saying that if Bennett's statement was a true reason for the diminished mortality, "then the large bleedings must have been an essential part of the cause of the larger mortality, and, even when apparently useful, must have been really inert, or, perhaps, only counteracted in their injurious effect by constitutional vigour, misunderstood by the practitioners." And he went on to argue that any such inference as that we "now possess pathological information sufficiently precise to entitle us to set aside the practical conclusions touching the power of bloodletting over inflammation, which our predecessors had drawn from their observations of disease, and which they attempted to establish as principles in therapeutics, . . . is a very erroneous one, and . . . may on some occasions be a very mischievous one."

In support of these views, Dr Alison first of all appealed to what he himself had already published as to the fallacy of medical statistics applied to this subject, and then went on to state that he had no doubt whatever as to bloodletting being a *summum remedium in maximis morbis*. This statement he supported by a relation of his experience in regard to pleurisy, pneumonia, bronchitis, acute meningitis ("often of scrofulous origin"), rheumatic affections of the heart, etc. He still held that in inflammations seen early, and accompanied with the inflammatory fever usually regarded as its natural effect, "bloodletting is the only effectual remedy, and it is *not safe to dispense with it*." Any change in the relations of inflammatory affections to bloodletting he ascribed to the fact that inflammation not only "often occurred *without such 'febri'e reaction,'* as Cullen and other authors have described as demanding and bearing the full bleedings, . . . but that it now *very seldom occurs with such symptoms*; and, further, that the constitutional fever attending it has very often more or less of the *true typhoid* character." In short, while admitting that inflammations could in many instances be treated successfully without bloodletting, and scarcely ever required anything but moderate or small bleedings, that was only because inflammations no longer possessed the true sthenic character they had in the days of Cullen and Gregory, but had changed their type and become asthenic and typhoid in character. From beginning to end it was a mere statement of opinion, founded on an experience of forty years, no doubt, but resting on no firmer basis than the misleading doctrine of *quia post eryo propter*.

In the discussion² which followed there was a general agreement

¹ *Edin. Med. Journal*, March 1856, p. 769.

² *Edin. Med. Jour.*, April 1856, p. 947.

in the statements of Dr Alison, but no other arguments were adduced, if we except an appeal to the varying types of exanthematic and other epidemic fevers as confirmatory of the possibility of a change of type in inflammation. Dr Bennett, who was not present at the meeting of the Society, replied in the course of the following session.

In his paper¹ he states—and you will note the contrast between 1847 and 1857,—“It is admitted by both parties that the practice of bleeding in acute inflammations has, within a recent period, undergone a great change; that whereas it was the rule to bleed early, largely, and often repeatedly, that now much bleeding is rarely practised, and is not necessary.” And he goes on to say that, according to Dr Alison, this remarkable change of treatment was due to a change of type of disease, while in his (Dr Bennett’s) opinion inflammation is the same as it had ever been, but the principles on which bloodletting and antiphlogistic remedies have hitherto been practised are opposed to a sound pathology, the only object of judicious medical practice being to conduct inflammatory disease to a favourable termination, to cut it short being impossible, and that all positive knowledge of experience of the past, as well as the more exact observation of the present day, alike established the truth of these propositions. These and some other subordinate propositions were enforced with all Dr Bennett’s well-known energy, and supported by ample statistics, which were now found not only sufficiently trustworthy, but fully to bear out all that had been said ten years before. The discussion which followed² was taken part in by Drs Alison, Christison, Gairdner, etc.; no new feature was elicited, and the arguments were simply repetitions of what had been already said, and were ably replied to by Dr Bennett. To this statement of Dr Bennett’s views Dr Alison replied through the press,³ and this paper was succeeded by a rejoinder from Dr Bennett.⁴ Dr Alison argued that while the *morbid anatomy* of inflammatory diseases undoubtedly entitles us to consider “exudation of decolorized lymph” as the essential characteristic of such diseases *post-mortem*, and that after this change had progressed during life to a certain extent, the case could no longer be benefited by bloodletting, and might easily be injured by it, yet that we possess no pathological information which entitles us to doubt that bloodletting in the early stage of the inflammation—when the stagnation in the capillaries and the exudation of lymph are only commencing—may arrest or greatly shorten those local changes, as well as most materially diminish the danger from the general disease.⁵ At the same time, there could be no doubt that of late years, and in various parts of the

¹ *Ed. Med. Jour.*, March 1857, p. 769. Read to the Society, 21st Jan. 1857.

² *Ed. Med. Jour.*, March 1857, p. 856.

³ *Op. cit.*, May 1857, p. 971.

⁴ *Op. cit.*, p. 995.

⁵ *Op. cit.*, March 1856, p. 772.

world, "inflammation seldom shows itself with such general symptoms as demand, or would justify, in the opinions of the practitioners treating them, or indeed would bear, the large bleedings which were formerly regarded as the appropriate remedy for it, which accordingly are seldom practised.¹ In support of these views Dr Alison appealed to his own experience, adducing also that of many of his compeers.² He also asserted that though Cullen and Gregory were not adepts in the modern methods of diagnosis, they were yet very good judges of the power of bloodletting in the treatment of such inflammations of the chest as they were in the habit of seeing.³ And to show what these were he quoted the case of a young man from whom Dr Gregory took ninety-seven ounces of blood within forty-eight hours—the last bleeding leaving the man in convulsions, the students rushed from the ward, and Gregory himself "had his own share of uneasiness." This, however, we may remark, was not an everyday case; Gregory himself acknowledged that. By-and-by he triumphed over it; he not only published it, but he boasted in his clinical lectures, as was told me many years ago by one who heard him, that "within a week it was heard of in Geneva," a great feat for those days. Alison concluded by stating that if "Dr Bennett's pathology leads necessarily to the belief that the principle in therapeutics which the great body of practitioners, since medicine has been a subject of reflection to mankind, have adopted in regard to the effect of bloodletting in the early stage of inflammatory disease, is false, I confess that I should think that a much better reason for setting aside his pathology than their therapeutics."⁴ Dr Bennett rejoined,⁵ that if bloodletting, to be efficient, ought, as Alison had stated, to be practised within the first three days, the records of the Infirmary would prove that bloodletting, when it was largely practised there, was very rarely had recourse to at that early period, even by Alison himself. Dr Alison was mistaken in thinking that he (Dr Bennett) took his pathology from the dead body; his observations were made on the transparent parts of living animals, and confirmed by the phenomena found to occur in patients affected with inflammatory disease, all of which were consistent and explicable by what was discoverable after death. He did not doubt that those medical men who practised these bleedings believed them to be beneficial, and that they saved life. He considered that the real test of a successful practice was not to be found in the relief of symptoms, but was unquestionably that which caused the fewest deaths, and promoted recovery in the shortest time. When inflammatory disease was treated by anti-

¹ *Op. cit.*, March 1857, p. 973.

² *Op. cit.*, May 1857, pp. 973, 982, etc.; and for many letters in support of the change of type theory, *op. cit.*, October 1857, p. 292.

³ *Op. cit.*, May 1857, p. 976.

⁴ *Loc. cit.*, p. 986.

⁵ *Op. cit.*, May 1857, p. 995.

phlogistics, the mortality was one in three; when, however, treatment was adopted which favoured the natural process of cure, the mortality was only one in twenty-one. These facts appeared to him irreconcilable with Alison's statements and opinions. He did not doubt that patients die from varying degrees of pneumonia; but these variations existed formerly as well as now, depended on the different amount of vital power possessed by each individual, and in no respect supported the theory of change of type. Dr Bennett argued that "an empirical treatment derived from blind authority, and an expectant treatment originating in an equally blind faith in nature, are both wrong; that a knowledge of physiology, pathology, and therapeutics, and not mere experience, is the real foundation for the practice of the medical art, and that a true experience can only have for its proper aim the determination of how far the laws evolved during the advance of these sciences can be efficiently applied to the cure of disease."¹ These statements may be regarded as embodying the gist of what has been called the bloodletting controversy. There was a great deal of controversial speaking at the meetings of this Society, and a still greater amount of controversial writing in the *Edinburgh Medical Journal*, but it was a mere reiteration of opinion on both sides, with some variations in the method of putting it. Dr Bennett and Sir Thomas Watson had a small passage-at-arms on this question,² and on the part of Professor Gairdner of Glasgow there was a more bitter attack and reply, in which the personal element was allowed to play only too conspicuous a part.³ Dr Gairdner was almost the only one who appealed to the history of medicine, showing that the disuse of bloodletting was only a revival of an old story, and not a new departure, and that however favourable the ultimate results of pathological and diagnostic researches may have been to a change of practice, that change was unquestionably not due to the leaders in these inquiries, but to far other men. And he brought forward proof to show that Dr Bennett owed but little to his views in pathology and diagnosis for his present opinions in bloodletting—a fact of which, indeed, no proof was requisite, for every one knew that Bennett's new pathology⁴ was published in 1844, yet he not only denounced what he called a "do-nothing practice" in 1847, but continued to practise like other people till 1856, when he was suddenly converted. Nevertheless, Bennett deserved great credit for the energetic manner in which he fought for the doctrine of non-interference when he had once received it, as well as for the rules he laid down as to the only true method of advancing the science and practice of medicine. So far as this

¹ *Op. cit.*, May 1857, p. 1000.

² *Ed. Med. Jour.*, June 1857, p. 1084 and p. 1088.

³ Gairdner's paper, *Ed. Med. Jour.*, Sept. 1857, p. 197. Bennett's reply, *op. cit.*, p. 229. Discussion in *Med. Chir. Soc.*, *op. cit.*, p. 369.

⁴ *On Inflammation as a Process of Abnormal Nutrition*, Edin., 1844.

Society was concerned, this controversy was now dropped, till in 1865 a paper was read which gave it the *coup de grace*. I shall refer to this in summing up; for the present, I must diverge into the cognate subject of the change of type in fevers.

The late Sir Robert Christison favoured this Society with a very carefully elaborated statement as to a change of type in fevers, based on his own experience of the previous forty years.¹ In our day, and with our very different pathological views, it is almost incomprehensible how any argument founded on a change of type in fevers could be admitted as proof of a change of type in inflammations also, and Dr Bennett, very wisely admitting some small degree of truth in the one, declined to acknowledge it to have any bearing on the other, or to agree that the change of treatment of fevers, which it was acknowledged had preceded that of inflammations, was in any way connected with it. Dr Christison's paper was indeed a very valuable contribution to the past history of fever epidemics, but, though received by most of the members of this Society as proof of change of type, it was not long of being challenged by him who was by-and-by to prove the great historian of fever, Dr Murchison.² Without entering into the question of whether the cases referred to were cases of distinctly different diseases, or merely varieties of one form of disease, Dr Murchison pointed out that the so-called synocha of 1817-20 was the fever now so well known as *relapsing fever*—no new disease even then, but one well known for over a hundred years, and always characterized by a small mortality. "One of the main arguments," says Murchison, "if not the principal one, urged by Dr Christison in favour of a change in the type of fever is, that in the epidemic of 1817-20 the practice of bleeding largely, so far from being injurious, as it would undoubtedly be in the fever which of late years has been most prevalent, was followed by the most favourable results. Thus he remarks, after speaking of drawing 'a legitimate allowance of thirty ounces (of blood) in all,'—this was from himself; Christison had always the courage of his opinions,—'And let it be remembered that we did by no means slay our patients by such bloodthirstiness. On the contrary, the mortality from the whole forms of fever collectively in that epidemic did not exceed one in twenty-two at any period, and was reduced to one in thirty as the epidemic spread and the remedy became more and more familiar.'"³ "But," adds Murchison, "when no bleeding has been resorted to, the mortality (of relapsing fever) has been smaller than under the heroic practice resorted to in Edinburgh during the epidemic of 1817-20."⁴ While even in those days, and taking the statistics of Welsh, the great apostle of bloodletting in fever, the heroic practice was by no means so suc-

¹ *Ed. Med. Jour.*, Jan. 1858, p. 577, and July 1858, p. 38; and for discussion, *op. cit.*, May 1857, p. 1034.

² *Ed. Med. Jour.*, August 1858, p. 97.

³ *Op. cit.*, Jan. 1858, p. 587.

⁴ *Ed. Med. Jour.*, August 1858, p. 98.

cessful as it was deemed. In treating his 743 patients in Queensberry House, Welsh detracted 10,166 oz. of blood, and applied 4364 leeches,¹ with this remarkable result, that—

| | |
|---------------------------------|------------------------------|
| Of 364 cases bled from the arm, | 20 died, or 1 in 18·2 |
| Of 189 „ leeches, | 10 „ „ 1 in 18·9 |
| Of 190 not bled in any way, | 4 „ „ 1 in 47·5 ² |

These are Welsh's own figures, and do not exactly bear out Christison's assertion. Moreover, the idea that the mortality diminished in 1818, because the profession had become more alive to the benefits to be derived from bloodletting, is explained by Murchison to have been really due to the displacement of typhus (a highly mortal disease) by relapsing fever, which is rarely fatal.³ When relapsing fever reappeared in 1843, for the first time since 1829, it was thought to be a fever entirely new to Edinburgh, but a reference to Christison's accurate descriptions of earlier epidemics speedily set that at rest, its characteristic lineaments were so distinct. Nevertheless, it is said by Christison never to have "presented the same strong phlogistic or sthenic character as in the earlier epidemics of 1817-20 and 1826-29; the pulse was neither so frequent nor so strong, the heat was not so pungent, the glow of the integuments was less lively and less general."⁴ Fortunately for scientific purposes, Dr Christison has translated this somewhat metaphorical language into words which, though less poetical, are more easily understood. He has told us that "the pulse ranged from 120 to 160, it might be large or it might be small; but if the latter, it was wiry; if the former, cordy—that is, always hard and incompressible." While "the heat of the body ranged from 102° to 105°, at times even to 107°."⁵ Well, Cormack,⁶ and Henderson,⁷ and Craigie,⁸ the three chief historians of the epidemic of 1843-44, are all agreed that the pulse ranged between 90 and 120, though it was sometimes as high as from 140 to 150, and was hard, wiry, and not easily compressed—the temperature ranging from 104° to 106° (but little attention was paid to temperature in those days, and we have almost no accurate information of this epidemic in this respect). These figures give no indication of any deficiency in "phlogistic or sthenic character," and the accuracy of the observation may be absolutely relied upon, Henderson's being most care-

¹ Welsh, *Practical Treatise on the Efficacy of Bloodletting in the Epidemic Fever of Edinburgh*, 1819, p. 186.

² Welsh, *op. cit.*, p. 184, and table xxii.

³ *Treatise on Continued Fevers*, London, 1873, p. 411.

⁴ *Ed. Med. Jour.*, Jan. 1858, p. 592. ⁵ *Ed. Med. Jour.*, Jan. 1858, p. 582.

⁶ *Natural History, Pathology, and Treatment of the Epidemic Fever at present prevailing in Edinburgh*, by John Rose Cormack, M.D., 1843.

⁷ "On some of the Characters which Distinguish the Present Epidemic Fever from Typhus," *Ed. Med. and Surg. Jour.*, vol. lxi., 1844, p. 201; read to Med. Chir. Soc., Dec. 6, 1843.

⁸ "Notice of a Febrile Disease which has prevailed in Edinburgh during the Summer of 1843," *Ed. Med. and Surg. Jour.*, vol. lx., 1843, p. 410.

fully made in order to contrast it with typhus fever, from which he proposed to differentiate it. For you must never forget that up till that time certainly Christison regarded relapsing fever as only a more sthenic form of typhus; we have his own authority for that.¹ In 1870 synocha—as Christison loved to call it—reappeared in Edinburgh, and spread all over the country; and when we examine the carefully recorded facts of this, the latest epidemic, we find them very closely to resemble those of the famous synocha of 1817. For in Edinburgh in 1870 the pulse ranged from 120° to 130°, while the temperature varied from 102° to 104°, and was occasionally over 106°; and from observations made in the London Fever Hospital, and by many accurate observers in Germany, we find that occasionally the temperature rose to over 108°.5.² Now, it may be difficult to define what a “phlogistic character” or a “pungent heat” of skin may mean, but the actually recorded facts teach us that the temperature of the patients in relapsing fever, in every epidemic from 1817 down to the present day, has been very much the same; and from what we know of fatal hyperpyrexia in fever, it could never have been higher than it was in the latest epidemic. So far, therefore, as either pulse-rate or temperature is concerned, there has been no decrease either in “phlogistic character” or “pungent glow” of skin. While if it be claimed that there has, nevertheless, been a loss of a subtle “something” which rendered it less amenable to treatment by phlebotomy, we have another answer to that than an absence of “phlogistic character.” Christison says, “In the epidemic now under consideration,” that of 1843-4, “there never was a question raised about the revival of bloodletting as a remedy, and the answer must be too apparent to require mention.”³ But Cornack tells us a very different tale. He says that bloodletting was resorted to with apparently beneficial results, but it was given up because “these beneficial changes were found to be not results, though they were the sequences of bloodletting, as was satisfactorily proved by the very same changes frequently occurring as suddenly and unequivocally in patients in the same ward, and affected in the same way, who were subjected to no treatment whatever.”⁴ A statement which has been confirmed as to a more recent epidemic by Sir William Jenner, who says, *apropos* of a case of relapsing fever bled in the London Fever Hospital, “Nature unaided by the loss of blood in many cases effected a much larger improvement in a much shorter space of

¹ Discussion on Professor Henderson’s paper, *Ed. Monthly Journal*, Feb. 1844, p. 177.

² “Relapsing Fever in Edinburgh,” by Claud Muirhead, M.D., *Ed. Med. Jour.*, July 1870, p. 1. Fox, *Med. Times and Gazette*, March 1880. Obermeier, “Ueber die Wiederkehrende Fieber,” *Archiv f. Path. Anatomie u. Klin. Medizin*, vol. xlvii. pp. 162 and 428. For many other authorities, vide Murchison’s *Treatise on Continued Fevers*, 1873, p. 355, &c.

³ *Ed. Med. Jour.*, August 1858, p. 592.

⁴ Cornack, *op. cit.*, p. 151.

time.”¹ The facts just narrated are sufficient, I think, to warrant the conclusion that the first notion of a change of type in fever was based upon the idea that relapsing fever was merely an unusually sthenic form of typhus. This idea was fostered by the results which seemed to follow an antiphlogistic treatment, results which emboldened Welsh to say that bloodletting “is of more value than all the other remedies which have been used in fever put together, and I am convinced that the bad or dubious success of bloodletting in fever has been entirely owing to the too sparing quantities in which it has hitherto been used in that disease.”² And yet Welsh’s own statistics show that the mortality of fever patients who were bled is more than double that of those who were not bled, and a further experience proved that the relief which seemed to follow bloodletting was but part of the natural history of the disease. It would be difficult to find a more remarkable instance of Bacon’s *idolon theatri*. The change of type in fever is a myth, yet some of the ablest minds in the profession clung to that myth while life remained.

Very similar is the history of change of type in inflammations. Phlebotomy was early brought into note by the extravagant reward bestowed upon him who has the credit of having first practised it;³ it rapidly produces decisive results, and even though it fail to cure it never fails to give temporary relief. It is a powerful treatment—a *summum remedium*—and whatever happens to the patient, it is always of advantage to the practitioner in a rude age or among ignorant people. “What devil is in this salt,” said an Afghan physician to Ferrier, “for of a hundred patients who have had it only one has survived?”⁴ Small wonder—the salt was cyanide of potassium; but the treatment showed power, and did the physician no harm in a region where weakness is the only unpardonable sin. And yet what science there was in the early ages was opposed to this heroic practice. Pythagoras, the earliest medical philosopher, who lived 500 years before Christ; Erasistratus, his grandson, and the great school which adopted their views, and flourished down to the second century of our era, continually protested against phlebotomy as a useless and injurious waste of life. The great and important influence of this school may be gathered from the virulence with which Galen attacked Erasistratus, who had been dead for 400 years, and his followers, who were then the most distinguished practitioners at Rome. His caustic diatribes prevailed; forced at first to flee from Rome, he ultimately returned triumphant, and, partly by the force of imperial favour, partly by the brilliancy of his special pleading, he succeeded in so firmly establishing phle-

¹ Jenner, *Med. Times*, vol. xxiii. p. 31, 1851.

² Welsh, *op. cit.*, p. 179.

³ Podalirius, 1134 A.C. He received the hand of his patient, the Princess Syrna, whom he bled for *concussion* of the brain, with the Carian Peninsula as her dowry.

⁴ “Caravan Wanderings in Persia, etc.,” *Blackwood's Magazine*, October 1857.

botomy as the rule of practice in inflammation, that almost down to the present day it has continued to be regarded as pre-eminently orthodox. Yet Erasistratus never ceased to have distinguished followers in all ages; and though the discovery of the circulation of the blood seemed to lend irresistible cogency to the arguments of Galen, the doctrines and the followers of Erasistratus still flourished, and were in as high estimation as those of Galen.¹ In the midst of this apparent chaos, where two of the most divergent systems of treatment prevailed, each of them based on opinion, and supported by experience, a seeming light at length arose. Towards the end of the seventeenth century the investigations of Bonetus began to confirm the statement, first made by Lælius-à-fonte, that the lungs were hepatized in cases of death from pneumonia. By-and-by the researches of Valsalva, Morgagni, and Lieutaud proved that hepatization of some part of the lung is invariably present in all who die with symptoms of pneumonia. And at last Cullen, having adopted Hoffman's doctrine of spasm of the arterioles as the cause of inflammatory congestion,² regarding hepatization as an effusion of blood into the parenchyma of the lung due to this congestion, and believing in bloodletting as the only efficient remedy for spasm, propounded the doctrine that free bloodletting was the sole remedy for inflammation, and that its efficacy depended mainly on the day of the disease on which it was commenced. Whether true or not, we now had a definite statement of the pathology of inflammation, and a system of treatment propounded which seemed exactly to meet the requirements of the case. Bleeding *usque ad deliquium* was a certain means of relaxing all external spasm; the spasm of the arterioles was bound to give way; the only question was, had we got the case early enough—if so, the inflammation must cease, it was jugulated. Nothing could be more plausible than the reasoning. For the first time, and as it seemed for all time, the treatment of inflammation was placed upon an irrefragible and thoroughly scientific basis. A greater than Galen had arisen, before whom all should bow, and to whom, for seventy odd years, almost all did do homage. And truly he deserved it. Think of the scanty material at his command, and how skilfully he used it—the admirable remedy he selected, so well fitted for its work, so unequivocal and so rapid in its action. If the disease was curable, it was cured at once, jugulated. If the remedy failed, it was used too late—

¹ An account of the literature of this subject will be found in a paper entitled "Hæmatophobia: an Historical Sketch," by George W. Balfour, M.D., *Ed. Med. Jour.*, Sept. 1858, p. 214, and somewhat more fully in *An Introduction to the Study of Medicine*, by the same author, Edinburgh, 1865.

² *First Lines*, 2nd ed., Edin., 1778, vol. i. § cexxxviii. p. 185, and § ccliv. p. 200. In a clinical lecture by Cullen on the case of Colin Reid, a very interesting account is to be found of his views as to the pathology and treatment of pneumonia. *Vide MS. lectures*, vol. iii. p. 588, in the library of the Royal College of Physicians, Edinburgh.

of that there was the never-failing proof to be found in the hepatization of the lung.¹ Among pathologists and therapeutists there has been none, I think, greater than Cullen. And now all this skilfully elaborated pathology, and the treatment founded upon it, have vanished from our ken, and a new generation of physicians has arisen who have never seen a man bled for pneumonia, and who probably scarcely know how to handle a lancet; perhaps they have never seen one out of an instrument-maker's, and yet in quite recent times the lancet was so universally employed for everything, that even a purge was said to be carried on its point. The change is most marvellous, and we can scarcely wonder that our immediate predecessors believed it could only be brought about by an entire change in the constitution of disease. And yet proof of the reverse lay ready to their hand. In the library of the Royal College of Physicians here we have seven volumes of MS. clinical lectures by Cullen, extending over ten years, from 1764 to 1774, and containing upwards of 200 cases. I have looked over the whole of them, but have been unable to find one single case of pleurisy or pneumonia which could be regarded as typical. Typical pneumonia must thus have been at least as rare a disease in Cullen's day as it is now. There are two, however, which Cullen at first thought might be peripneumonic. The first was admitted on the fifth day of the disease, the other on the ninth. In the first the pulse is said to have been neither full nor hard, yet she was bled thrice. Cullen subsequently acknowledged that the case was simple catarrh with a stitch in the side. The second case is illustrated by an elaborate clinical lecture on the pathology of pneumonia, its clinical history and treatment.² This patient's pulse was 126, no other characteristic being mentioned. He was bled twice on the day of admission, and once on each of the two following days. The case was afterwards acknowledged to be one of fever with inflammatory affection of the bronchial glands. The other cases recorded are very much like our ordinary hospital cases, only there are a good many cases of intermittent fever, which was then endemic among us. There is also a large number of cases of inflammatory fever and catarrh, in whom the pulse-rate is rarely mentioned; in a few it is said to be under 100, and it is very often said to be soft and not always full. Yet none of them escaped the inevitable bleeding on two or three days successively after admission, though this is repeatedly stated to have been upon the ninth or tenth day. Now and then, too, Cullen naively congratulates himself on the case not having turned out nervous fever, in which case, he adds, "*The bleeding would*

¹ *First Lines, ed. cit.*, vol. i. p. 267. Here Cullen describes pulmonary hepatization as a termination of inflammation quite peculiar to pneumonia, and in a MS. note on p. 268 he adds, "I do not remember to have seen one single exception to this in fatal cases of pneumonia."

² *Vide* case of Colin Reid, just referred to.

have been pernicious." At another time he tells of a case of pneumonia which had been treated as fever, and he expresses a regret that he did not recognise the true nature of the case during life, as he would certainly have bled him, and perhaps done him good. I need scarcely say more.

There are in London several MS. clinical lectures of Cullen and Gregory, and the cases recorded are said not to differ from the ordinary run of cases in the London Hospitals at the present day. One case of double pneumonia, admitted on the fourth day after a rigor, and bled once before admission. His pulse was full and soft; he was bled five times in two days, dying within forty-eight hours.¹ Small wonder, you may be inclined to say; but bleeding in the hands of a skilful master was not so fatal a treatment even when pushed to an extreme. We have in the library of the Royal College of Physicians here one MS. volume of clinical lectures by Gregory, and two volumes of notes of cases taken from clinical lectures, extending over three sessions, 1771-72, 1779-80, 1780-81. In them I found seven cases of pneumonia. One woman, Betsy Moffat, was admitted on Jan. 3rd, 1772. She had her first rigor on the 24th Dec., and had been bled before admission. On admission she was found to be "breathing with the utmost difficulty, and there appeared to be a considerable quantity of matter in the breast which was rustling in her throat, and which she appeared scarce able to throw up. I then," says Gregory, "diagnosed a peripneumony, or inflammation of the lungs; and from its having continued several days, I thought it was probable that an effusion had begun to take place in her lungs, and that the chances were much against her. The circumstance of delirium was also extremely unfavourable. That is one of the terminations for peripneumonia. A translation to the head very generally proves fatal." Yet Gregory bled her to eight ounces, twice over; purged her well, and gave her a vomit of tartar emetic. Her pulse, which was 134, had fallen on Jan. 5th to 96; on the 6th her head was leeched for throbbing of the temples; and on the 9th she was dismissed cured. On January 16th she was readmitted with a relapse of her former symptoms—her pulse 130. She was treated as formerly with repeated venesections and emetic doses of tartrate of antimony. On the 18th she was insensible, her pulse 104 and feeble, with a rattle in her throat like one dying. Gregory stimulated her out of this state with stroug sack whey, hartshorn, and warm fomentations to her legs. Before he left the ward he was able to leech her head to relieve a pain of which she complained. She afterwards had four more ounces of blood taken from the arm, and a blister applied to the side, and was dismissed cured on January 25th.² I confess I am lost in wonder alike at the boldness and

¹ *British and Foreign Medico-Chirurgical Review*, vol. xxii. pp. 32, 33, and 34.

² A pretty full account of these cases will be found in a paper entitled "Cullen and Gregory upon Change of Type in Inflammation," by George W.

the success of the practice. But there is nothing either in this case or in any of the others, which I need not further detail, to indicate the existence of a disease of a more sthenic or more strongly phlogistic character than we are in the habit of seeing and treating every day. Nay, more, Cullen in a copy of his *First Lines*, interleaved and annotated by himself, has a note in his own handwriting to the effect that the detraction of "ζiv." of blood is sufficient to produce fainting in many persons. Yet such persons bear, he adds, subsequent bleedings better, so that they may be even larger than the first, and at last such a quantity may be procured as the symptoms of the disease may seem to require. And in a MS. note he adds, "This fact and rule hold very universally."¹

According to Cullen's pathology, then, pneumonia was a congestion of the lungs produced by spasm of the arterioles, running on to rupture of the vessels, and effusion of blood into the parenchyma, generally fatal on the ninth day if not previously relieved.² Those who died beyond the ninth day had gone on to suppuration, and an exceptional few of them recovered, but they were beyond the pale of treatment.³ Now and then, however, it appeared that rupture and effusion of blood did not take place till much later, and bleeding was sometimes successful so late as the twentieth day.⁴

According to Cullen's therapeutics, there was only one effectual means of relieving the spasm and preventing effusion of blood, and that was bloodletting, which, though only entirely and certainly successful in the first few days of the disease, might be so exceptionally at any date. It was therefore right to employ it in every case, and at any period of the disease, so long as the patient could bear it. And I am sure you will agree with me, that those who believed in this pathology were right to practise as they did, and that both physician and patient acted wisely, in accordance with their lights, in pushing this *summum remedium* even to an extreme. For when the pathology and the remedy were both so plain, no one, I think, could blame either physician or patient for grasping at a remedy in itself so certain, the only doubt lying in the stage already reached by the disease. In this light the old dogma, *Melius anceps remedium quam nullum*, acquires quite a new and a reasonable significance. Unfortunately this most remarkable pathological hypothesis was baseless: it fell, and with it the treatment founded on it, so soon as the profession recognised that the natural history of pneumonia

Balfour, M.D., *Ed. Med. Journal*, September 1865, p. 213, read to the Medico-Chirurgical Society, June 7th 1865; and also in the *Introduction to the Study of Medicine*, already quoted.

¹ *Op. cit.*, § cccxlviii. pp. 279 and 280.

² *Lecture on Colin Reid*.

³ *Lecture on Colin Reid*, and *First Lines*, vol. ii. § dcccxxviii. p. 231 *et seq.*

⁴ *First Lines*, vol. i. p. 280, MS. note, *alio cit.*

was not in accordance with it. So soon as it was clearly understood that bloodletting could not prevent hepatization, and that patients did recover from pneumonia without bloodletting, then bloodletting was doomed. It had long been known to be full of danger in the hands of all but the most skilful; the very symptoms produced by it simulating closely those for which it was supposed to be needful.¹ I do not think that the profession was sorry to get rid of it. Those who had long practised it were unquestionably astounded: the most certain thing in medicine had suddenly become a delusion, everything seemed slipping from them. We cannot help being sorry for them, and yet it was their own fault. A knowledge of the history of medicine would have enabled them to judge rightly the will-o'-the-wisp which led them astray, and landed them at length in such a quagmire of a theory as change of type, for which there is not, and never has been, even a semblance of a basis.

Dr P. H. Watson moved a vote of thanks to the President for his most able and interesting address. He was sure he expressed the feeling of every member of the Society when he said how much gratified they had been by an address which gave a most interesting *résumé* of two great advances in the practice of the profession which had been made within the recollection of most of the older members. It used to be the duty of the mover of a vote of thanks to the retiring President for his address and conduct in the chair to request that the address be printed. Now, however, thanks to the energy of *Dr Craig*, that duty was no longer required. Their Transactions being printed, they would have an opportunity of giving the address the leisurely study it would so well repay.

Mr Joseph Bell seconded. He claimed the privilege of doing so because of the fact that he had once been bled from the arm *secundum artem*.

Meeting II.—December 5, 1883.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

I. VOTE OF THANKS TO THE RETIRING PRESIDENT.

The President, *Dr Littlejohn*, on taking the chair for the first time, thanked the Society for the honour he had received in his election to office. It would be his endeavour to maintain the interest and reputation of the Society in the same efficient manner as had been done by his predecessors. It was one of his pleasures

¹ Marshall Hall, *op. cit.*

in taking the chair that he was to succeed his old friend Dr George Balfour, to whom he would propose a hearty vote of thanks for the excellent way in which he had performed his duties during the past two years.

This was agreed to unanimously, and the vote of thanks conveyed to Dr Balfour.

II. ELECTION OF NEW MEMBERS.

The following gentlemen were elected Ordinary Members:— Henry Newcombe, M.D., F.R.C.S.E., F.R.S.E., Edinburgh; Francis Troup, M.D., Edinburgh; Russell E. Wood, M.B., C.M., Edinburgh; J. MacDonald Brown, F.R.C.S. Eng., Edinburgh; James W. B. Hodsdon, M.D., M.R.C.P., Edinburgh; John Haddon, M.D., Edinburgh; G. Sims Woodhead, M.D., F.R.C.P. Ed., Edinburgh; Thomas Francis Spittal Caverhill, M.D., F.R.C.P. Ed., Edinburgh; R. A. Landie, M.A., B.Sc., M.B. and C.M., Edinburgh; Arthur W. Hare, M.B., C.M., Edinburgh; Edwin Bailey, M.B., C.M., Edinburgh; Alexander Black, M.B., C.M., M.R.C.P., Edinburgh; Harry Melville Dunlop, M.B., C.M., Edinburgh; George Andreas Berry, M.B., F.R.C.S. Ed., Edinburgh; Hamilton Wylie, M.B., C.M., Edinburgh; James Bennet, M.B., C.M., Leith; A. Douglas Webster, M.B., C.M., Edinburgh; Robert W. Philip, M.A., M.B., C.M., Edinburgh; William McNeil, M.D., L.R.C.S. Ed., Stranraer.

III. EXHIBITION OF PATIENTS.

1. *Dr J. Jamieson* exhibited a male patient who illustrated the good effects of drainage in empyema. He was a shoemaker, who suffered in the early part of the year from an attack of pleurisy with effusion. This was aspirated, and a large quantity of pure serous fluid removed. In a fortnight the operation was repeated, but the fluid was no longer pure. It presented a turbid appearance, and contained pus. The case was then very unpromising. There was much difficulty of breathing, profuse perspiration, and great debility. An incision was made in the posterior aspect of the chest, a drainage-tube inserted, and the pleural cavity repeatedly washed out through this with warm water well coloured with Condyl's fluid. Some time later a swelling appeared anteriorly, evidently connected with the pleural cavity. This was also opened, and the pleura washed out through it. He was afterwards sent to the country with the tubes *in situ*. Having caught cold in Aberdeen, he went to the infirmary there, was admitted, and had the tubes taken out. After a few weeks' residence he came south well, and now bore little trace of his severe illness. The chest on the affected side was only one inch less than on the other side.

2. *Dr Shand* showed a female patient who had recently suffered from a FISTULA OF STENO'S DUCT. The natural opening of the duct

had closed, but its patency was re-established by means of a probe, while the fistulous opening was kept closed by collodion. There was no salivary calculus, and in a short time the fistula healed.

IV. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Mr Duncan* showed the PARTS REMOVED IN A CASE OF NEPHRECTOMY. So far as the specimen was concerned, it was an ordinary case of pyo-nephrosis. The patient was sent to him by Dr Angus Macdonald, with a history of an attack of cystitis during her pregnancy more than a year before coming under treatment. Dr Macdonald had tapped the swelling, and found it to contain sero-purulent fluid and urea. The diagnosis was therefore easy. An incision was made in the middle line of the abdomen. The peritoneum, which was granular over the tumour from inflammatory action, was divided to the outer side of the colon, and the pyo-nephrosis easily stripped from the surrounding textures. The renal vessels were tied with silk. The treatment of the ureter was, he considered, the mistake of the operation. It was divided pretty low down between two ligatures, the orifice left being tipped with chloride of zinc to prevent any septic infection of the peritoneum. For some days after the operation there was a considerable amount of distention of the bowel, and undoubtedly some peritonitis, with a moderately high temperature, but nothing septic. A little discharge was sucked out of the glass drainage-tube left in the pelvis, but it contained no organisms. On the eighteenth day the patient was allowed to sit up—a little too soon, perhaps, because down towards the side of the bladder a slight induration remained. After she got up this increased in size and was painful. A fortnight later an attempt was made to reach it by the aspirator through the vagina unsuccessfully, but a few days after it was incised through the vagina, just too late, as it had opened into the bowel, and she died seven weeks after the operation. The abscess formed in the stump of the ureter. The lesson to be learned from this was a different mode of dealing with the ureter. It had been proposed to take it to the anterior part of the wound and allow it to drain there. In this case he (*Mr Duncan*) believed that he should have done so, but he missed the meaning of the previous history. For months before there had been no pus in the urine, which meant that the ureter was occluded at the bottom.

2. *Mr A. G. Miller* showed a KNEE-JOINT removed by amputation for advanced disease. The joint had disappeared entirely, there being no synovial membrane nor cavity except the cavity of an abscess. It also showed a complete disorganization of the joint—so much so that recovery could not have taken place without some operation. The patient being eighteen, the epiphysial junction was well marked. Had excision been performed, the

epiphysis would have been removed, and further growth of the bones prevented. Amputation was performed because of the incurability, because of the dislocation backwards, and because of the diminished size of the bones. The disease had lasted twelve years, and the limb had not grown properly. It was three inches shorter than the other. Had excision been done, the shortness would have been increased to four and a half or five inches. He first complained of the disease when six years of age. In a short time he seemed to recover; but four or five years ago he fell and fractured his thigh, after which the disease in the joint got worse till he came into hospital. The operation was done on the 16th November, and the wound had healed, except where the drainage-tube had been.

3. *Dr Blair Cunynghame* exhibited, for Mr Lawson Tait, eight specimens of diseases of the Fallopian tubes and ovaries—"pyosalpinx." (a.) A specimen showing CHRONIC INFLAMMATION AND SUPPURATION OF BOTH FALLOPIAN TUBES. The symptoms came on after an attack of gonorrhœa. There was constant pain, amounting to agony, just before and during menstruation. The organs in the pelvis were quite fixed and exquisitely tender. Marital intercourse had been entirely suspended for three years. The operation was difficult and tedious, the bleeding being very free. The incision was 2 inches in length, and a drainage-tube was inserted. (b.) Two small DERMOID TUMOURS, causing in two separate patients constant and intense pain. One of them was sent to Mr Tait as a case of pyosalpinx. Both recovered, and are quite cured. (c.) A DOUBLE PYOSALPINX from a very bad case, the patient almost dying at the time of operation. She had now completely recovered. (d.) A DOUBLE PYOSALPINX. A small myoma was also present in this case. The patient had made a good recovery. (e.) A DOUBLE PYOSALPINX from what Mr Tait thought was the worst case he had ever had. The contents of the tubes were horribly fœtid, and had burst into the cavity during the detachment of adhesions. Mr Tait was of opinion that he had not taken enough trouble in cleansing the cavity, for the patient had nearly died. Convalescence had been slow, but she was now in good health. (f.) A PAIR OF OVARIES from a young woman (æt. 19), hypertrophied, cystic, and the white tunic greatly thickened, with abundance of new fibre cells. There was no pain, but tremendous menorrhagia. Everything having failed to arrest the hæmorrhage, the appendages were removed. These cases were rare, and the preparations were, in Mr Tait's opinion, very valuable (see Mr Tait's book, last edition, pp. 115, 116). (g.) The rarest of all pathological conditions of the ovary, a CHRONIC ABSCESS, removed on account of the persistent pain. (h.) A DOUBLE PYOSALPINX. In this case there was a very small amount of pus in the tubes. The patient had been under Mr Tait's care for eight years, and every-

thing had failed to relieve the intense menstrual pain. She had resisted his proposals to operate for five years, then consented, and was now well.

4. *Dr Kirk Duncanson* showed (a.) A MOLLUSCOUS TUMOUR removed from the external auditory meatus in May of the present year. This was the second tumour of the kind removed from the same meatus. The first, a portion of which and a microscopic preparation of it were also shown, had been excised about eight years previously, and the case recorded in the *Edinburgh Medical Journal* for November 1877. The first tumour rose from the anterior wall of the meatus and passed into the tympanum, pushing aside the tympanic membrane, and, pressing on the portio dura, caused paralysis of the right side of the face. On the second occasion the tumour arose from the posterior wall of the meatus, encroached on the mastoid cells, and pushed inwards the dura mater in the neighbourhood of the lateral sinus. There were two small points of ulceration just over the sinus, from which the danger of hæmorrhage was very great. When the tumour was removed, the cavity in the mastoid cells would have admitted of a large walnut. (b.) Two specimens of KERATOSIS OBTURANS, and a plug of wax, ceruminosis obturans, to show the distinction between them and the cholesteatoma (a). (c.) A plug of COTTON-WOOL taken, on 4th November 1883, from the meatus of a patient, who asserted that it must have lain there for over forty years. It had not given rise to any distention of the canal. (d.) Two specimens of POLYPOID GROWTHS which occurred in patients, the subjects of acquired syphilis, after the removal of a hardened plug of wax. There was some little bleeding, as often happened in these cases, and a clot having formed in each, became organized. One of the growths was removed about three weeks after its formation, the other a few days after. In the first case a small vessel, passing from the membrana tympani into the growth, bled on its removal. (e.) A portion of a MUTTON BONE coughed up by a patient when having applications made to her naso-pharynx for catarrh of the Eustachian tubes. She said it must have lain in her lungs four years.

V. EXHIBITION OF MICROSCOPICAL SECTIONS AND DRAWINGS.

Dr Byrom Bramwell showed (a.) a CONGENITAL MALFORMATION OF THE GRAY MATTER in the upper cervical region of the spinal cord. There was a mass of gray matter detached from the rest and placed between the anterior cornua. (b.) A MULTIPOLAR NERVE-CELL with TWO NUCLEI. This was a rare condition, the only one *Dr B.* had himself seen. (c.) A preparation from a case of myelitis, showing a VACUOLATED NERVE-CELL. The cell was completely filled with small vacuoles. (d.) A section of the cord in a case of COMPRESSION MYELITIS. A tumour pressed on the cord in the

dorsal region, and its whole tissue was in consequence degenerated, the nerve-structure being entirely destroyed. (c.) SECTIONS OF THE SPINAL CORD IN MYELITIS. Four of these showed the appearances of the cord of a man who had been admitted to one of the Liverpool hospitals under Dr Dickinson. The patient had been healthy till three weeks before, when he was seized with paraplegia. There was nothing to account for the condition, except that he had had a severe attack of syphilis two years previously. When the cord was examined, patches of myelitis were seen scattered in various parts, most marked in the dorsal region. In two of the sections shown the bloodvessels were enlarged, and in one there were also extravasations of leucocytes into the surrounding tissue of the gray matter. In another hypertrophy of the axis cylinder processes was observable. Lastly, there was a section from the medulla of a man who had died under Professor Annandale's care. He had been cut for stone, and soon after the operation was seized with hemiplegia. Sections of the medulla at first showed nothing to the naked eye; but after careful examination a little localized softening, without any alteration in colour, was observable. Under the microscope there was a well-marked lesion following the course of the bloodvessels, and hypertrophy of the axis cylinders.

VI. ORIGINAL COMMUNICATIONS.

1. ON THE EARLY PUBLICATIONS OF THE MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

By WILLIAM CRAIG, M.D., F.R.C.S. Ed., etc., Lecturer on Materia Medica, Edinburgh School of Medicine.

IN 1882 the Medico-Chirurgical Society of Edinburgh entered on a new era by the publication of an *annual* volume of Transactions, and the *presentation* of the same to every Ordinary Member of the Society. In the volume for that year no notice was taken of the fact that the Society had at an early period of its history published several volumes of Transactions. The object of this paper is to supply that defect, and to give the members some idea of the early publications of this Society.

The Society was founded 2nd August 1821, and the first volume of Transactions was published in 1824. It contains only original communications, and evidently includes most of the papers read during the first two sessions of the Society. It is a volume of 725 pages, and was published at 18s., the publisher being Mr Adam Black of this city. Of this volume 750 copies were printed. A second volume of Transactions was published by Mr Black in 1826. It contains the papers read during the third and fourth sessions of the Society. It contains 435 pages. Of this volume

500 copies were printed. A third and last volume of Transactions was published in 1829, and evidently includes the papers read during the fifth and sixth sessions of the Society. It consists of 676 pages, and was also published by Mr Black. Of this volume only 300 copies were printed. The published price of the three volumes was £2, 16s.

I have not been able to ascertain the precise terms on which these early publications of the Society were offered to the members; but it is very evident, from the gradual diminution of the numbers printed, that financially their publication was not a success, and had to be abandoned. This is very much to be regretted, for the first three volumes contain very valuable communications; and had the Society been able to continue the publication of the papers read before it in a collected form, it would have conferred more benefit upon its members and the profession generally.

In the able *résumé* of the work of this Society by a former President, the late Dr P. D. Handyside, in his *Jubilee Chronicon*, reference is made to many of the papers contained in these volumes; but it appears to me that it might be both interesting and profitable to the members to know more of the very able communications which were presented to this Society during the early years of its existence, many of these papers written by men whose names will be had in everlasting remembrance. I shall endeavour to give you a brief *résumé* of these early publications.

There are in these three volumes 89 original communications, of which fully the half treat of medical subjects, and 17 of surgical subjects. The other papers relate to physiology, pathology, poisons, comparative anatomy, and malformations.

Amongst the medical papers, 13 treat of general diseases. Amongst these, scrofulous and tubercular diseases have a prominent place. Six of the papers are devoted to these affections. Two of these papers are by Professor Alison,—one entitled “Observations on the Pathology of Scrofulous Diseases, with a View to their Prevention,” with beautifully coloured illustrations; and the other, “Additional Cases and Observations illustrating the Origin of Tubercles.” These are very able papers, and the author endeavours to establish the following conclusions:—(1.) That the general prevalence of the scrofulous diathesis is to be ascribed rather to the modes of life which an advanced and artificial state of society implies than to the circumstance of climate; and (2.) That scrofulous tubercles may be, and often are, deposited in consequence of inflammatory action.” Another is by Dr John Abercrombie, “On the Nature and Origin of Tubercular Diseases,” in which he expresses the opinion that “deposition of albumen is the origin of tubercular disease.” Another paper is by Dr William Moncreiff, entitled “Case of Tubercular Disease of the Peritoneum and Omentum.” Two papers are by Mr William Wood, entitled “Observations on Painful Subcutaneous Tubercle.”

Mr John Howship, London, a corresponding member of the Society, has an interesting and instructive paper, illustrated by an excellent drawing, entitled "Case of Mollities Ossium, with the Appearances on Dissection." There is also an excellent paper on Melanosis by Mr William Cullen and Mr Robert Carswell, in which the authors maintain that melanosis and cancer are "diseases quite of a different nature." There is an interesting paper, entitled "History of a Case of Anæmia," by Dr J. S. Combe, who has so lately passed from amongst us, full of years and of honour. Two papers treat of "Purpura Hæmorrhagica,"—one by Dr Ebenezer Gairdner, and the other by Dr P. Fairbairn. Mr William Hamilton, surgeon, H.E.I.C.S., communicated a very exhaustive and interesting paper on that remarkable disease known as Beriberi, for which he strongly recommends bloodletting. Dr Alexander Macaulay contributed an interesting paper entitled "Cases of Persons Struck by Lightning." In these cases bloodletting was the favourite remedy. Eight papers treat of diseases of the heart and bloodvessels. The first paper in Vol. I., and which appears to have been the first paper communicated to the Society, was by Dr John Abercrombie, read 5th December 1821, entitled "Contributions to the Pathology of the Heart." This is a learned and able paper, in which the author treats the subject thus:—(1.) Inflammatory Affections; (2.) Organic Affections; (3.) Rupture; and (4.) Displacement of the Heart. Another paper will be read with much interest, entitled "Case of Carditis attended by unusual Symptoms; with an Account of the Appearances on Dissection, Death having occurred from another Cause after the Cure of the Original Disease," by Dr John Gairdner. Mr J. H. Wishart, Surgeon to the King in Scotland, records two cases of heart disease, in both of which he made post-mortem examinations. In one he found that "the right ventricle was occupied by a polypous concretion extending into the auricle, which it filled so completely as must have been a great impediment to the circulation from the veins into the auricle and from the auricle into the ventricle." In the other case "the valves were in a thickened, cartilaginous state." Both of these preparations were deposited in the museum of the Royal College of Surgeons. Two papers treat of aneurism of the aorta, one by Dr Thomas Molison, complicated with disease of the spinal cord, which he attributes to the aneurism. The other paper is by Dr Archibald Inglis. In his case the aneurism ruptured into the œsophagus. This case is well recorded, and the paper is interesting as being one of two in these three volumes of Transactions whose author is still alive. Two papers on the "Sudden Spontaneous Obstruction of the Canals of the Larger Arteries, with some Observations on the Processes employed by Nature to prevent or arrest Hæmorrhage from lacerated Arteries," by John W. Turner, Professor of Surgery to the Royal College of Surgeons, are communications of very great value. Professor Andrew Duncan, jun., communicated an interesting paper on a

“Case of Inflammation of the Cephalic Vein, which terminated Fatally; with the Appearances on Dissection.”

Of diseases involving the œsophagus, trachea, and lungs, we have four papers. Dr William Cumin, Glasgow, communicated a paper entitled “Remarks on Stricture, with three Cases of Stricture of Œsophagus,” in which he gives three causes of strictures:—(1) inflammation; (2) degeneration of the mucous lining or the walls of a tube into various morbid states, more especially carcinoma; and (3) spasm. Dr David Hay communicated a “Case of Dysphagia, with Abscess involving the Œsophagus, Trachea, and Lungs.” Dr James Pitcairn related an interesting case of “Empyema successfully treated by Paracentesis Thoracis;” and Dr Dumbreck communicated a paper entitled “Case of Emphysema without Local Injury.” Five of the papers relate to affections of the abdominal viscera,—one on “Chronic Gastritis,” by Mr William Brown, one of two of the original members of the Society who still survive. Dr James Molleson’s paper, “Notes of some Remarkable Cases,” will be read with much interest. One case illustrates stricture of the ileum, with artificial anus at the umbilicus; but, notwithstanding, there were frequent evacuations by the rectum. Dr John Gairdner communicated two valuable papers on an infantile disease in which erosions and perforations of the alimentary canal were found after death. Dr A. H. Renton has an interesting paper entitled “Observations on the Dysentery of Madeira,” in which he says, “The great frequency of the disease, there can be little doubt, is attributable chiefly to a want of proper clothing. As regards treatment, he strongly recommends “mercury given boldly and perseveringly, until the mouth become decidedly affected.”

Four of the papers treat of diseases of the nervous system. Mr J. P. Rhind, surgeon of cavalry, H.E.I.C.S., has a very interesting case of phrenitis with great cerebral congestion, which he successfully treated by opening the radial artery. This case of phrenitis occurred in his native assistant, who at midnight was taken suddenly and dangerously ill. Dr Rhind found him in a low, muttering delirium; his pulse quick, hard, and small; his eyes much dilated and inflamed. He goes on to say, “I immediately sent for my lancets; but his pulse sank so rapidly, that before I had time to open a vein of the arm it could not be felt at the wrist, and not one drop of blood would flow; nor were my attempts with the jugular more successful. I now, without the smallest loss of time, divided the temporal artery, in which a very feeble pulsation could still be felt. From it a very small stream flowed, and that there might be as sudden a depletion as possible, I also divided the corresponding artery of the opposite side; but even this was insufficient to arrest the rapid progress of the disease, and before four ounces could be abstracted it entirely ceased. His extremities became cold, and neither was his breathing nor the pulsation of

his heart perceivable. He was to all appearance dead; so much so, that the attendants had already begun to close his eyes and stretch his limbs, when, happening to observe a case of scalpels upon the table, the thought suddenly struck me that, as one last and only hope, I would try the effect of opening the radial artery. I immediately snatched up one of the scalpels, cut down upon the artery, and, upon its being laid fairly bare, made a small puncture in it with the point of a lancet. A few drops only of blood oozed from the wound; presently, however, in small uninterrupted stream, as from a vein, and nearly of as dark a colour; the stream gradually enlarged, till at length, the arteries recovering their natural impetus, the blood was propelled per saltum, and the same instant the poor fellow opened those eyes which every one present supposed had been closed for ever. He recovered."

Another of the papers relates an interesting case of chronic hydrocephalus, by Mr Charles Miller. The patient died at the age of 16½ years. The disease was first noticed when the child was 11 months old. On examination after death, the "convulsions of the brain were floating like loose intestine, and defied dissection; when touched they went nearly into a gelatinous mass." There was a quantity of water within the cranium measuring 8½ pints; and yet it is stated that "his memory was particularly correct; indeed, his mental faculties were completely unimpaired." There is a curious paper by Dr John Storer, Nottingham, communicated by Dr Abercrombie, in which the author gives "some account of a cerebral affection arising during the course of pulmonary consumption, and arresting the further progress of that disease." The cerebral affection referred to consisted generally of abscesses or ulcers. A paper of great interest, both historically and otherwise, by Mr William Wood, entitled "Observations on Neuroma, with Cases and Histories of the Disease," occupies a prominent place in Vol. III.

Four of the papers treat of fevers. Dr Robert Hamilton, surgeon to the Magdalene Asylum, communicated a very interesting paper on a fever which occurred in that asylum in April 1821. The paper illustrates very forcibly the influence of panic in propagating infectious diseases. The disease spread with alarming rapidity among the inmates, until Dr Hamilton effectively stopped the progress of the disease by telling them that such rapid spread of the disease was never heard of, and insisted that the fumigation had fortified them against the most virulent contagion. In Vol. II. there is an excellent account of the yellow fever which appeared in the Queen's Regiment in Barbadoes in 1816 and 1817, by Dr Alexander J. Ralph, assistant surgeon to the regiment. Dr W. H. Ruan communicated an interesting account of a very peculiar contagious epidemic disease, a disease which was *sui generis*, but evidently a fever, accompanied with

an exanthematous eruption, which made its appearance in the island of St Croix in October 1827, having been imported from the neighbouring island of St Thomas. Mr Walsh, assistant surgeon of the 89th Regiment, communicated an account of the fevers and other diseases which attacked the British troops during the Burman war, a paper interesting and instructive. Two papers treat of epidemics of erysipelas—the one by Dr John Stevenson of Arbroath, in which he seeks to prove the contagious nature of that disease, and the other by Dr W. Gibson of Montrose.

Two papers treat of syphilis—one by Sir George Ballingall, entitled “Remarks on the Cranium of a Man who died of Syphilis,” and the other by Dr John Wilson, Hull, on the “Natural or Spontaneous Cure of Syphilis,” for which he recommends “simple external applications, a rigid antiphlogistic regimen, including a complete state of rest.”

Five of the papers are devoted to therapeutics. Two of these treat of the use of tobacco in tetanus. They are by Dr Thomas Anderson of Trinidad, who tried the tobacco treatment of this affection, and with marked success, both in the traumatic and in the idiopathic form of the disease. He used a strong decoction of the fresh leaves, with which he fomented the muscles contracted, and afterwards cataplasms of the leaves boiled till quite tender and applied over the parts. The warm bath, into which some of the decoction was thrown, was used every three hours, and a clyster of the same decoction was given twice every 24 hours. He preferred the native tobacco, which, he says, is milder than the Virginian variety. For the decoction he recommended four pounds of the dried plant to be boiled for an hour in eight gallons of water. There is a very able paper by Dr James Begbie on the “Sedative Powers of *Datura Stramonium*, with Cases.” He used an extract from the seeds. The dose was one-fourth of a grain, repeated every third or fourth hour during the day, and in no instance increased beyond half a grain. Dr Begbie states that in no case was any bad effect produced. Dr William Young contributed a paper on the “Use of Ergot in Cases of Tedious Labour.” He strongly recommended the drug in these cases, and also for checking hæmorrhages. The remaining paper is an interesting one by Dr John Davy, entitled “Observations on James’s Powder and the Antimonial Powder of the London Pharmacopœia.”

Three papers treat of poisons. The most important of these is an able and learned paper by Sir Robert Christison, entitled “An Account of several Cases of Poisoning with Arsenic, in illustration of the Delicacy of the Chemical Evidence and Weight of the Evidence drawn from Symptoms.” Dr A. H. Renton of Madeira contributed a valuable paper on the “Poisonous Effects produced by swallowing the Seeds of *Datura arborea*,” a plant very abundant in many parts of the

island. The symptoms observed were those usually seen in poisoning with the atropææ, flushed countenance, dilated pupils, and wild, wandering expression. The other paper is on the "Deleterious Effects of the Smoke of Coal," by Dr John Gairdner, a paper of much interest, in which he relates the cases of six individuals subjected to its influence, all of whom recovered.

Only one paper treats of obstetric medicine. It is by Dr William Young, who records a case of extra-uterine gestation, a subject attracting considerable interest among obstetricians at the present day.

Among the seventeen surgical papers are several of great value. One of the most interesting of these occurs in the second volume, entitled "On the Use of a New Substance, Silkworm Gut, for securing divided Arteries," by Mr George Fielding, surgeon, Hull. The members will pardon me if I draw their special attention to this long-forgotten but most interesting and instructive paper. Mr Fielding's great desire was to find a substance "likely to be absorbed or dissolved in the animal fluids, and yet to possess sufficient tenacity to remain long enough upon a divided artery to secure the union of its coats." With the view of solving this problem he made experiments in 1823 (sixty years ago) on some animal substances which he thought likely to be absorbed, and on the suggestion of his assistant, a Mr E. Heseltien, he tried the substance known by the name of silkworm gut—a substance much used by fishermen at the end of their lines, and which may "be procured at any of the shops where toys or fishing-tackle are sold." I quote Mr Fielding's own words:—"Previous to employing the substance upon the living body, I cut out portions of a vein from a subject, and, after tying knots of the gut upon them, immersed them in a bottle containing fresh serum. The bottle and its contents were kept at a moderate heat for two or three days, and at the end of that time the knots were apparently as secure as when first made. This experiment showed that the substance retained its tenacity long enough to produce union of the coats of the vessel to which it was applied at a temperature nearly corresponding with that of the human body."

Mr Fielding's first experiment in actual practice was on 10th August 1823. He says, "I extirpated the left testicle of a patient æt. 30. The gland was large, weighing upwards of 12 ounces. A considerable portion of the scrotum was removed at the same time with the testicle. The spermatic artery was tied naked with a very small silk ligature. Three pretty large arteries of the scrotum were secured by means of the gut, and the ends cut off close by the knots.

"The divided edges of the scrotum united by the first intention, except where the ligature of the spermatic artery was left out; a slight suppuration took place about this, which came away on the

eleventh day. We saw no more of the ligatures upon the other arteries. The patient was discharged cured on the eighteenth day; and there can, I think, be little doubt, if the other substance had been used upon the artery of the cord, the whole wound would have healed by the first intention."

He relates a considerable number of other cases in which he tried it, some of them being amputations above the knee, and in all of which it answered remarkably well. Mr Fielding stated that the vessels were secured effectually; that in no case was any appearance of the knots seen, though carefully looked for at every dressing; and that in no case was there any abscess or other inconvenience following its use.

He concludes by saying, "The vessels are as secure, the wounds heal sooner, and the patients suffer less pain during the cure than by the methods in common use. The subjects were not at all selected for the purpose, but were taken as they occurred; some of them were anything but favourable for the trial of any new process."

Mr Fielding was thus half a century in advance of his fellow-surgeons; and it is somewhat remarkable that such an improvement in surgery as that suggested by Mr Fielding, communicated to this Society, and published in its Transactions, should have been entirely overlooked by the surgeons of later times.

There is an interesting paper by Dr J. Boggie, Surgeon to the Forces, on "Hospital Gangrene, with reference to the Disease as it appeared in the British Army during the Peninsular War." The same author has also an excellent paper on "Gunshot Wounds of the Extremities," illustrated by a case. Professor Andrew Duncan, jun., contributed two valuable papers on "Diffuse Inflammation of the Cellular Substance." Professor James Russell communicated an interesting paper on a peculiar affection to which the bones of the head are liable. Mr J. F. Macfarlan, the founder of the well-known chemical firm of J. F. Macfarlan & Co., related the case of a severe injury of the head, with supposed transverse fracture of the base of the cranium, from which the patient recovered.

Dr Richard Huie has a very interesting case of the removal of a polypus of great size from the root of the tongue. A ligature was tied round the pedicle, and on the fourth day the tumour came away entire. The tumour is in the Museum of the Royal College of Surgeons. It was oval in form, weighed an ounce, measured 5 inches in its long and 4 inches in its short circumference.

Two of the papers relate to ophthalmic surgery,—one on "Exanthematous Ophthalmia," by Mr James Wardrop, Surgeon Extraordinary to the King; the other on "Chronic Inflammation of the Iris," by Mr Alexander Watson, beautifully illustrated with coloured drawings. Mr Watson remarks in this paper, "that although several of the patients affected with this disease were or had been treated by courses of mercury, the remedy considered most likely

to arrest its progress, this desired event did not take place." Two of the papers treat of "Hernia,"—one a remarkable case of crural hernia, by Sir George Ballingall, in which he strongly recommends an early resort to operation wherever any vestige or remains of hernia exist in combination with obstinate obstruction of the bowels. The other paper is a "Case of Strangulated Umbilical Hernia," in which the operation was performed with success, by Dr James Simson.

Two papers refer to cases of extraction of calculi from the female bladder by dilatation of the urethra,—one by Dr Robert Hamilton, in which he used to a small extent the bistoury in addition to dilatation. The calculus weighed 1 ounce. Its long circumference measured $4\frac{1}{2}$ inches, and its short $1\frac{1}{4}$ inch. She was completely cured. The other case was recorded by Dr Alexander Ramsay, Dundee. In his case the urethra was gradually dilated for some weeks, and the calculus was successfully extracted without cutting. The calculus measured $5\frac{1}{2}$ inches in its long circumference, and $3\frac{1}{2}$ inches in the short, and weighed $7\frac{1}{2}$ drachms. Another paper is by Sir George Ballingall, entitled "Case of the High Operation of Lithotomy, in which unusual Difficulty was experienced in the Extraction of the Stone; with a Drawing." The difficulty was caused by the stone being large, friable, and encysted in the fundus of the bladder.

Two of the surgical papers relate to affections of joints,—one by the anatomist Knox, on a "Remarkable Alteration of the Structure of the Patella," produced apparently by the presence of a loose cartilage within the cavity of the knee-joint, with beautiful illustrations. The other paper is on the "Dislocation of the Hip and Shoulder Joints," by Dr Adam Hunter.

Physiology.—Eight of the papers are devoted to this branch of medical science. There is a very able and interesting paper on sympathy by Professor Alison, with special reference to the doctrines of Mr Charles Bell, in which he expresses his firm conviction, *first*, that what are called sympathetic actions are, in general, actions caused by sensations; and *secondly*, that no anatomical explanation can be given of the fact that certain sensations act on certain nerves only. Another, by Dr John Davy, "On the Effects of the Sun's Rays on the Human Body," is a paper of great interest. The author treats (1) of the changes connected with the discolouring effect of the sun's rays; (2) of the part of the skin in which the discoloration takes place; (3) of the cause of the change of colour, and of the manner in which it operates; and, lastly, of the consequences of the discoloration of the skin. A third paper is by Dr C. J. B. Williams, on "Respiration and Animal Heat, with Experiments," from which the author concludes that the change effected on the blood in the lungs consists in the acquisition of oxygen and the loss of carbonic acid. In regard to the power by which this change is produced, the author believed that it arises principally, if not entirely, from a chemical affinity

subsisting between the gases and the blood, and that in the process of arterialization the oxygen absorbed displaces the carbonic acid gas from venous blood. A fourth paper is on "Cutaneous Absorption, with Experiments," by Dr John Dill, from which he draws the following conclusions:—(1.) That the body in general increases in weight when in the bath; (2.) that when its weight is unaffected by immersion, something must have been absorbed by it to answer the natural expenditure of the system, which has been apparently suspended during the experiment; (3.) that when its weight is diminished, this arises either from an inactive condition of the cutaneous absorbents, or from the temperature of the bath being so high as to accelerate the action of the heart and arteries, and thus increase the perspiration; (4.) that the additional weight acquired during immersion cannot be accounted for either on the supposition of an increased pulmonary inhalation or of a diminished evacuation by the skin; and lastly, that the most simple and satisfactory method of accounting for this increase of weight is by admitting the doctrine of cutaneous absorption. A fifth paper is by Dr John Davy, on "The Effect of Removing Atmospheric Pressure from the Fluids and Solids of the Human Body," in which he contends that in the healthy body there is no free air contained in any of its fluids or solids. A sixth paper, also by Dr Davy, is on "The Specific Gravity of Different Parts of the Human Body." A seventh paper, by the same, will be read with great interest. It is an account of some experiments on different parts of the human body—(1) on the effects of desiccation; (2) on the effects of tannin. Another paper of great value is entitled "Observations on the Causes of the Sounds produced by the Action of the Heart," by J. W. Turner, Professor of Surgery to the Royal College of Surgeons. He is disposed to conclude that the contractions of the auricles and of the ventricles communicate only one sensation, the first of the two sounds of the heart, and that the second sound is caused by the impulse occasioned by the falling back on the pericardium of the relaxed heart in its diastole, after it has been elevated or moved from its place in the systole, and possibly also in part by the ventricles attracting the blood by suction into their cavities.

Six papers treat of pathological subjects. Two are by Dr George Kelly of Leith. The one gives an account of the appearances observed in the dissection of several persons whose bodies were found near Leith during a severe snow-storm in November 1821, and who had died of starvation and cold. The other is entitled "Reflections on the Pathology of the Brain." This second paper is a supplement to the first. In these papers we have a careful record of many experiments on the bloodvessels which go to the brain, and the author concludes by saying that "nature has guarded with peculiar care the brain and its vessels against such accidents from repletion and depletion; and that while the structure of this organ

remains *healthy and unchanged*, and *its vessels sound*, those causes are little capable of causing plethora, congestion, effusions, or comatose conditions." Sir George Ballingall showed to the Society a skull which exhibited some remarkable morbid appearances, and gave an account of it and of the person from whom it was obtained. There is a good drawing of this skull accompanying the paper. Another paper of great interest is by Dr John Davy, entitled "Observations on the Temperature of the Human Body after Death." On one occasion he observed a temperature of 108° , and in another it was 113° . He advances the opinion that these high temperatures were generated during life. In the case of the highest temperature the body was examined three and a half hours after death, the temperature of the room being 86° . Dr John Davy has a very interesting paper recommending sulphurous acid as a preservative fluid for anatomical specimens. He shows that it is a powerful antiseptic and possesses many qualities not possessed by other fluids. He recommends it, also, to the botanist for preserving delicate plants. The remaining paper on this branch of medical science is by Mr James Brown, surgeon, Dominica. This is a paper to illustrate certain morbid preparations which he presented to the museum of the Royal College of Surgeons. Some of these specimens are very rare, if not unique. Amongst them is a case of rupture of the heart; another is a case of death from lumbrici. The lumbrici had taken possession of the stomach and intestines, and had penetrated into the liver. In the liver they were found in the vessels of the vena portæ; whilst another case was that in which the skin of a negro had changed from its natural black to a white colour.

There is one paper devoted to comparative anatomy, in which we have an excellent description of the viscera of the common swordfish, *Xiphias gladius*, Lin., by Dr Robert E. Grant, Professor of Zoology in the London University.

The remaining six papers are devoted to malformations. One of these is an interesting case of "Hydrocephalus with Bifid Brain," by Professor Andrew Dunean, jun., with a description of the malformation by Dr John Gordon. The infant, a female, was born hydrocephalic, and lived seven months. Another of these papers is by the celebrated anatomist Knox, entitled "Case of a Congenital Disease or Malformation of the Thigh Bone," illustrating the pathology of interstitial absorption of the *cervix femoris*. There is a very interesting case of "Malformation of the Heart," by Dr W. F. Holmes, Montreal, communicated by Professor Alison. This was a case of imperfect double heart. The following is the course of circulation in this curious case, who died at the age of 21 years:—"The blood, entering the right auricle by the two cavæ, passed almost entirely into the left ventricle, a small portion probably finding its way into the left auricle. A part of the blood would pass, during the diastole of the ventricles, from the left into

the right ventricle, and be propelled through the lungs, to be returned into the left auricle." A good drawing of the malformation accompanies the paper. Another of the malformations is that of an infant in which the stomach terminated in a *cul de sac*, by Mr John Crooks, surgeon, Kilmarnock. This case is not unlike one which I met with in my practice a few years ago, an account of which I published in the *Edinburgh Medical Journal* for August 1881. In my case the *cul de sac* was in the ileum. Mr Crooks's description of the rest of the alimentary canal applied also to my case—"The intestines, extremely small and contracted, were gathered like a bunch of worms." Dr Charles Hastings of Worcester gave a description of a monster in which the upper and inferior extremities were entirely wanting; a good plate illustrates this paper.

The last of the papers on malformations is by Dr Andrew Berry, and is that of a so-called twin-monster. The children (Chinese) were females, and lived till they were nearly 7 years old, when the death of the one caused the death of the other. They were united together exactly as were the Siamese twins, and, like them, had only one navel. A good drawing of this monstrosity accompanies the paper. This, as in all similar cases, is evidently the result of the splitting of a single ovum, and in no case the result of the union of two ova.

I have thus endeavoured to give the Society a brief, and, I fear, very imperfect sketch of the papers contained in the early publications of the Society, but I have said enough to show the members of the present day that from the very first the communications read before the Society were of great value, many of them written by men of whom Edinburgh and the profession are so justly proud.

I would add in conclusion that the first volume contains a list of members. That list is as follows:—2 hon. members, 85 ordinary members, and 107 corresponding members. Of these only two survive—Dr Robert Renton and Mr William Brown.

The President, in conveying the thanks of the Society to Dr Craig, said that many of the older members knew the three volumes, and thought highly of them. They were occasionally to be picked up at bookstalls, and were well worthy of perusal.

2. THE FOLD OF THE NATES.

By J. SYMINGTON, M.B., F.R.C.S. Ed., M.R.C.S. Eng., Lecturer on Anatomy, School of Medicine, Edinburgh.

THE "fold of the nates," or the "gluteal fold," is stated in almost all the text-books on anatomy to correspond to, or to be formed by, the lower border of the gluteus maximus, and in no English work with which I am acquainted are these assertions disputed. Thus Holden, in his *Landmarks, Medical and Surgical*,

third edition, 1881, says:—"The deep furrow, termed the fold of the buttock, which separates the nates from the back of the thigh, corresponds with the lower border of the gluteus maximus."

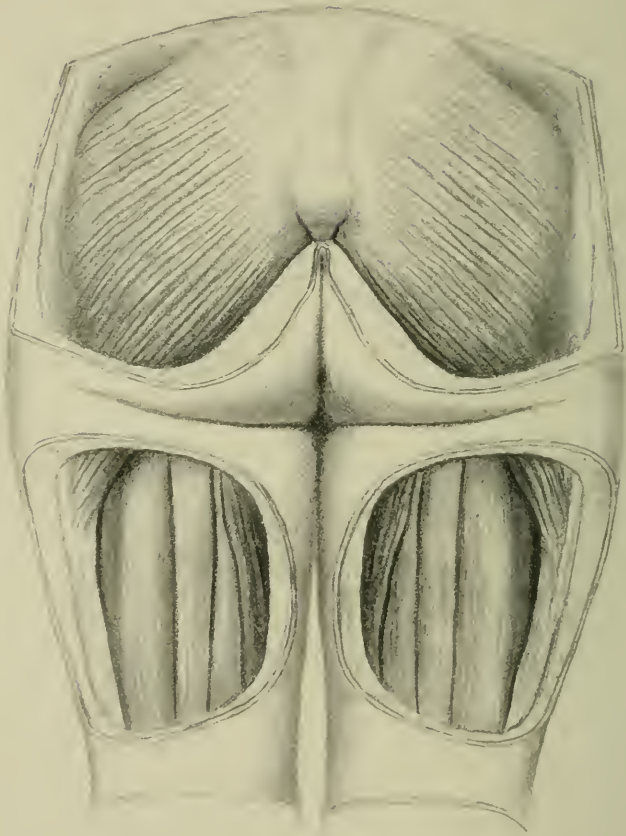
In Quain's *Elements of Anatomy*, ninth edition, 1882, vol. i. page 233, the lower border of the muscle is said to "lie in the fold of the nates," and at page 690, under the section devoted to superficial and topographical anatomy, it is stated that "the region of the hip, gluteal region or buttock, extends from the subcutaneous iliac crest and the origin of the gluteus maximus above, to the fold of the nates, produced by the thick lower margin of the gluteus maximus, below."

It is quite unnecessary to quote from further authorities, but I may mention that statements practically identical with those of Holden or Quain will be found in the works of Ellis, Gray, Heath, Wilson, Chiene, Allen, Treves, Reeves, and in Todd's *Cyclopaedia of Anatomy and Physiology*.

With reference to foreign authors I can find no allusion to the subject in Sappey's *Traité d'Anatomie*. In Cruveilhier's *Traité d'Anatomie*, fifth edition, 1877, the lower border of the muscle is said to form a very marked prominence. From the context it is evident that the prominence he refers to is the upper boundary of the gluteal fold, as he points out its surgical importance in connexion with the diagnosis of disease of the hip-joint, &c. Tillaux in his *Traité d'Anatomie Topographique*, troisième édition, 1882, says:—"It is the inferior border of the muscle (gluteus maximus) which determines the formation of the gluteal fold, and consequently the fold moves up and down with that muscle."

Henle, in his *Handbuch*, states that its lower border corresponds pretty accurately to the transverse fissure which separates the gluteal region from the back of the thigh. In Roser's *Surgical Anatomy*, translated by Galton, the relation between the two is still more sharply defined. He says—"The boundary between the nates and the back of the thigh is marked by the gluteal fold, which lies parallel with the edge of the gluteus, and is a guide to the position of the margin of the muscle."

A different, and I believe more accurate, account of the gluteal fold will be found in Luschka's *Anatomie des Menschlichen Beckens*, 1864. In giving the boundaries of the gluteal region (page 32), he points out that its inferior one—the gluteal fold—does not correspond, as he states it is erroneously asserted to do, with the lower border of the gluteus maximus, but passes from the inner circumference of the ischial tuberosity outwards, in a curved line, towards the great trochanter. He appears to consider the fold to be due to a pad of fat, and bands of connective tissue which pass from the skin through the fat to the ischial tuberosity and fascia lata. On page 138 he has a woodcut, in which he represents a view of the buttocks. On the one



side a dissection of the gluteus maximus and upper part of hamstrings is seen, on the other side no dissection has been made, but the prominence of the nates and the situation of the gluteal fold are indicated. It shows the difference in position and direction of the gluteal fold as compared with the lower border of the gluteus maximus. I have not seen a reference to Luschka's views in any work, yet the incorrectness of the usual statements can easily be demonstrated. I have taught views practically similar to those of Luschka for the last three years, although it was only recently, in looking up the literature of the subject, that I read his description.

The lower border of the gluteus maximus passes from the coccyx downwards and outwards to about the juncture of the upper with the middle third of the shaft of the femur. As it passes rather more downwards than outwards the border is more internal than inferior. Taking the lower border as straight, we found it formed an angle of about 145° with the mesial line of the trunk. The gluteal sulcus, on the other hand, passes outwards and slightly upwards. It is continuous internally with the groove between the inner side of the thigh and the perineum, and extends outwards below the ischial tuberosity towards the lower part of the great trochanter. It usually reaches about half-way across the back of the thigh, and gradually disappears as it passes outwards; occasionally there are one or more transverse folds below it. Close to the middle line the gluteal sulcus will be found, in an average adult, three or four inches below the muscle; but, owing to the lower border of the muscle inclining downwards and outwards, while the gluteal fold extends transversely outwards, the two meet a little external to the middle of the lower border of the muscle, and then the fold passes on to the posterior surface of the muscle. The outer end of the lower border of the muscle reaches two or three inches lower down than the outer end of the gluteal fold. As it is usually the custom in the dissecting room to first place the body in the lithotomy position and dissect the perineum, the normal relations of the fold are apt to be destroyed before the gluteal region is dissected. One of the best methods of showing the relation between the two is that which I have adopted in the Plate.

In this case I carefully fixed the skin in the position of the gluteal fold with several long pins, and then exposed the gluteus maximus, except where the gluteal fold crossed it. The subject was a well-developed adult female. I have followed this plan in several bodies, both male and female, with practically identical results, except that in a very muscular adult male the gluteal fold was found to cross on to the gluteus maximus a little nearer the middle line, and to extend across the muscle nearly at right angles to its fibres.

The want of correspondence between the two is also readily

seen in sagittal sections through the gluteal region and upper part of thigh.

It is, however, by no means necessary to make dissections to prove this, as it can easily be demonstrated on the living body. In a moderately muscular man, with a limited amount of subcutaneous fat, the lower border of the gluteus maximus can be clearly made out, especially when the muscle is contracted. Through the kindness of my friend Dr F. Godfrey, senior house-surgeon with Professor Annandale, I had an opportunity of examining a young man in whom the lower border of the muscle was readily traced. The inner part of the lower border of the muscle, which forms the posterior boundary of the ischio-rectal fossa, bulges inwards when the muscle contracts, and by the simultaneous action of the two glutei the two nates are approximated, and the cleft between the coccyx and anus is deepened. This part of the muscle is, in fact, closely related to the posterior part of the natal cleft. The border of the muscle can be traced outwards over the ischial tuberosity, and under the gluteal fold. The portion of the lower border, which extends from the level of the gluteal fold downwards and outwards to the femur can be felt, and in favourable subjects seen, in the contracted condition of the muscle. If a person standing erect contract his gluteus maximus the fold becomes a little more evident, but is not much affected. If he flex one limb, and support himself mainly upon the other, the fold on the flexed limb becomes less evident, lower than the other, and inclined downwards and outwards. The fold on the other side becomes more marked. The fold gradually disappears in the progress of flexion, and when the thigh is at about a right angle with the trunk it is no longer visible.

With regard to the causes of the gluteal fold the usual idea that it is formed by the gluteus maximus is a natural deduction from the belief that the gluteal fold corresponds to the lower border of the muscle. I trust I have proved this to be erroneous, and I think an examination of the two will show that the gluteal fold is essentially independent of the gluteus maximus. This anatomical error has often led surgeons to attribute the tendency to disappearance of the fold in hip-disease to atrophy of the gluteus maximus, but, I believe, the majority of surgeons more correctly regard the changes in the fold to be dependent upon the flexed position of the limb. I have found the gluteal fold well-marked in cases of great emaciation and general atrophy of the muscles of the body. Thus, it was well defined in a man aged 37, 5 feet 6 inches in height, suffering from advanced pulmonary phthisis, who weighed only 6 stone 4 pounds. Although the fold varies in these cases, yet it certainly has no definite relation to the degree of development of the gluteus maximus.

The causes given by Luschka, to which I have already

referred, are undoubtedly important factors in its production; but there are one or two others which assist in its production. The prominence of the lower and inner part of the nates is largely due to the ischial tuberosity, and the fold is always best marked where it passes outwards below it. The amount of flexion which can occur at the hip-joint with the knee flexed, and slight pressure exerted upon it with the hand, equals 145° to 150° (Morris). It is obvious that the skin which is put upon the stretch in flexion of the thigh is relaxed in the extended condition, and thus a fold naturally tends to occur below the projecting ischial tuberosity. _____

Dr Watson was of opinion that *Mr Symington* had made too much of what he supposed to be an error on the part of anatomists and surgeons. He was not aware that it was ever really taught that the fold of the buttock was formed by the margin of the gluteus maximus muscle; but as the line of the fold of the nates crossed the line of the inferior border of the gluteus maximus, there was no doubt that there was an intimate relation between them. This connexion was quite as definite as that of a given degree of latitude to a given degree of longitude, and the point of correspondence of the fold and muscle did form a definite guide for the incision to expose the sciatic nerve. *Mr Symington* had certainly shown very clearly that to say that the two lines were identical was absurd; but he did not think that any one who based his anatomical knowledge on the experience of the dissecting-room instead of the cabinet ever believed that the connexion was more than he had indicated.

Mr Miller had some time ago the privilege of hearing from *Mr Symington* his views on this subject, and of seeing them demonstrated in his (*Mr Miller's*) wards. He at once believed in them; and, notwithstanding *Dr Watson's* remarks, he had no hesitation in saying that he had been taught that the lower margin of the gluteus maximus was the same as the gluteal fold. He had even heard this said in the theatre of the Infirmary, when the fold was pointed out as one of the guides to the sciatic nerve in the operation of nerve-stretching. If he understood aright, *Mr Symington* ascribed the fold to the prominence above it of the tuber ischii and to the natural fold that took place in the change of position of the limb, the superficial structures of the thigh being relaxed when the limb was straight, and put on the stretch when it was flexed. He thought *Mr Symington* mentioned to him that, as a rule, the fold did not extend deeper than the skin and superficial fascia. If this was so, then the fold must be due to something existing in connexion with the skin and fascia. *Mr Symington* had told them that the gluteal was continuous with the inguinal fold. This could be ascertained by any one as a fact. The two met at the upper part of the ramus of the pubis. They knew that the superficial

fascia was attached to the pubis and its ramus and along Poupart's ligament. This same attachment, which was of importance in connexion with the surgical anatomical subject of extravasation of urine, was of importance in this connexion. He believed that the fold was produced by a drawing on or straining of this attachment, and not by a relaxation, as Mr Symington held.

Mr Symington said that, in spite of what Dr Watson had said, it was an undoubted fact that anatomists and surgeons had taught that the lower border of the muscle corresponded with the gluteal fold. He held that it had more to do with the formation of the natal cleft. Although the two did not correspond, yet as they crossed one another in the position of the incision for the stretching of the great sciatic nerve, the fold did at that point serve as a guide to the lower border of the muscle.

Meeting III.—January 16, 1884.

Dr D. WILSON, *Vice-President, in the Chair.*

I. ELECTION OF NEW MEMBERS.

The following gentlemen were elected Ordinary Members:—Joseph Carne Ross, M.D., F.R.C.P. Ed., Penzance; William Russell, M.B., Edinburgh; George Dickson, M.D., F.R.C.S. Ed., Edinburgh; Thomas Wyld Pairman, L.R.C.S. and P. Ed., Biggar.

II. EXHIBITION OF PATIENTS.

Dr Argyll Robertson showed TWO PATIENTS, one of whom illustrated the good effects of a NEW OPERATION FOR ECTROPION, the other the condition of the lid two days after operation. The principle underlying the operation was to impart to the tarsal cartilage an inward curvature; for all methods of treating such cases which did not restore to the tarsal cartilage its normal curvature would certainly fail in effecting a radical cure. The apparatus employed consisted of, first, a piece of waxed silk thread about a foot in length, to either extremity of which was fixed a moderately long, slightly curved needle; second, a piece of sheet lead about an inch in length and a quarter of an inch in breadth, with the extremities rounded and the edges smoothed; and third, a piece of fine indiarubber tubing. In performing the operation one of the needles is introduced through the skin at a distance of about a quarter of an inch to the outer side of the centre of the lid, and about a line from its margin. It is then drawn through the conjunctival surface, re-introduced at the fold of reflexion between the palpebral and ocular parts of the conjunctiva, pushed

downwards beneath the skin of the cheek to a point about one inch or one and a quarter inch below the margin of the lid, where it is drawn out. The other needle is made to follow a similar course about a quarter of an inch to the inner side of the centre line. The piece of lead, having a curvature similar to that of the globe imparted to it, is then fixed beneath the ligatures on the conjunctival surface, and the indiarubber tubing under them on the outer surface, after which the free ends of the thread are tied over the lower part of the tubing. The object of the lead is two-fold, to impart the proper curvature, and to prevent the ligature cutting or otherwise injuring the conjunctiva. The tubing prevented injury to the skin. Six eyelids have already been operated on, and the result has been highly satisfactory. Part of the good results following the operation are due to the suppuration which takes place in the track of the ligature, this being followed by cicatrization, which helps to keep the parts in position.

III. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Mr A. G. Miller* showed (*a.*) a portion of the spinal column from a case of FRACTURE OF THE CERVICAL VERTEBRÆ. The patient was admitted to hospital on 8th December 1883, suffering from a cut in the head. He was very drunk, and no particular facts in regard to the case could be made out at the time. In the course of a few hours it was ascertained that he was suffering from paralysis and anæsthesia as far up as his umbilicus, with the exception that the penis was sensitive. By 11 P.M. the anæsthesia had reached to the third rib, and involved the part of the inner aspect of the upper arm corresponding to the distribution of the internal cutaneous nerve. The penis then became insensitve, but up to the last the man felt distinctly when his bladder and rectum were full, though he could not pass his water nor control his anal sphincters. He died on 14th December, six days after admission. After death, when rigidity had disappeared, the neck was carefully examined and manipulated for crepitus or preternatural mobility, but neither was made out. The autopsy showed extravasation into the muscles of the neck posteriorly, fracture of lamina of sixth cervical vertebra with separation of the spinous process, and, further, a very distinct fracture of the body of the same. (*b.*) THE BONES OF THE ARM FROM A CASE OF AMPUTATION AT THE SHOULDER-JOINT for osteo-myelitis occurring after compound fracture of the humerus. Six days before admission the patient had sustained an injury from a waggon passing over the arm, just above the elbow, and causing a compound fracture. The case was treated in the country. Suppuration occurred, followed by osteo-myelitis. It was somewhat unusual to amputate at the shoulder for an injury at the elbow, but in this instance he could not have gone lower than the surgical neck. The nature of the disease, combined with the fact that an

injury of childhood had left nothing but cicatricial tissue, led him to disarticulate. Erythema of the side of the arm and shoulder, which was spreading on to the side of the chest, was present at the time of operating, but disappeared very shortly (about twelve hours) after the operation was performed, and did not recur.

2. *Dr C. W. MacGillivray* exhibited, (*a.*) for Dr Heron Watson, THE RIGHT ARM, SCAPULA, AND HALF OF THE CLAVICLE of a gentleman about 50 years of age, removed on account of an enormous enchondroma involving the upper part of the arm and shoulder. The patient had suffered from similar tumours of both hands for many years, and at one time the middle finger of the right hand had been amputated by the late Professor Syme on account of its inconvenient curled up position in the palm. The other fingers of both hands were also affected, but caused little inconvenience. After the operation he went to India, where he carried on his usual avocations, and was able to write, ride, and play lawn tennis until quite lately. Eighteen months ago he noticed a slight swelling in the upper part of the arm, and this had steadily gone on increasing. The elbow-joint also became preternaturally mobile, and was deformed by several nodular masses. He, however, allowed the tumour of the shoulder to increase to an immense size, until, indeed, the arm became practically useless, and the pressure on the axillary plexus occasioned great pain. He then came home, and was seen by Dr Heron Watson, who recommended the entire removal of the affected parts. The arm, scapula, and the outer half of the clavicle were accordingly amputated about three weeks ago. The patient never had a bad symptom. The dressing was not changed for a week, when all except the opening for the drainage-tube was found to have healed by first intention, and the patient was now perfectly well. (*b.*) Also for Dr Watson, A CYSTIC TUMOUR OF THE RAMUS OF THE JAW, taken from a woman on whom he had operated twenty-four years previously for a similar tumour affecting one-half of the body of the jaw. The cyst was filled with a glairy, gelatinous-looking fluid. The curious point was its recurrence after so many years. (*c.*) A PECULIAR BODY, the nature of which he did not know, found along with several others in the pleural cavity of a patient who had died from a pelvic abscess following an operation for stone in the bladder. They were only discovered on clearing out the cavity of venous clots and blood which had escaped from the engorged pulmonary vessels, so that their origin must remain as uncertain as their nature. The condition of the thoracic organs was perfectly healthy. They were about the size of beans, were covered with a smooth, serous-like membrane, and were calcareous in the centre.

IV. EXHIBITION OF INSTRUMENTS.

Mr Macdonald Brown exhibited TWO CLAMPS FOR ENTERECTOMY, one designed and used by Mr Treves of the London Hospital, the other by Mr Bishop of Manchester. Having demonstrated the method of application of both instruments, he stated that Bishop's clamp was a modification of Treves's, and that the advantages claimed for it by its inventor were—that it was more easily and expeditiously applied; entailed less meddling with the mesenteric attachment of the bowel; and, lastly, that it allowed the operator to test carefully the results of the operation, as regards patency of the gut, etc., after the divided ends of the intestine had been sutured together.

V. EXHIBITION OF MICROSCOPICAL SECTIONS AND DRAWINGS.

Dr Byrom Bramwell showed (a.) a transverse section through the cord in the lower cervical region, showing the CENTRAL CANAL OBLITERATED (filled up) by cellular elements, also a longitudinal section through the obliterated canal. (b.) A transverse section through the lumbar enlargement of the spinal cord in a case of TUBERCULAR MENINGITIS and peripheral myelitis; a sketch of the anterior median fissure and surrounding parts from the same; a section through the cord and membranes of the same, showing the layer of peripheral myelitis under a higher power. (c.) A SECTION through the spinal cord in a case of spinal meningeal hæmorrhage (taken from the body of a lunatic).

VI. A DEMONSTRATION OF SOME OF THE METHODS OF CULTIVATING MICRO-ORGANISMS.

Dr G. Sims Woodhead gave a demonstration of some of the methods of cultivation adopted for the growth of micro-organisms outside the body. He brought before the Society the various pieces of apparatus used, and a number of specimens of "pure cultivations" of micro-organisms, drawing particular attention to those growing on Koch's peptonized meat-gelatine. The first nutritive medium mentioned was the potato, which had been sterilized by boiling. On specimens of this there were various bacilli and micrococci growing, some of special interest, such as *B. anthracis*, the micrococcus of osteo-myelitis, pink yeast, etc. The method of preparing the peptonized meat-gelatine was described as follows:—Take of minced beef 2 lbs.; distilled water, 2 litres. Pour the water over the beef, and allow to stand in a cool place for 24 hours. Remove the fat from the surface, and then squeeze out the fluid in a press, pouring in distilled water to make up the two litres. To this fluid add 100 grams best gelatine, 10 grams of pure dried peptone, and 5 grams of common salt; dissolve, strain the

mixture, and neutralize with a saturated solution of carbonate of soda. The whole is placed in a four-litre flask, the mouth of which is stopped with a plug of cotton-wadding, and then kept at a temperature of 100° C. (in steam) for 30 minutes. The next day it is again steamed for 30 minutes, and again neutralized, if necessary, or even rendered slightly alkaline. Filter through a heated funnel (copper sheath, with glass inside, the water between the two being kept at a temperature of 80° C. or 90° C.) containing filter-paper, into small quarter or half litre flasks. Have these about half full. Steam these flasks containing the gelatine for 20 minutes; repeat this the next day, and the two succeeding days steam for 15 minutes each. This gelatine is most frequently used in test-tubes. Both flasks and test-tubes must be plugged with cotton-wadding and then exposed to a temperature of 170° C. for one hour before the gelatine is put into them. Each test-tube should contain about 10 cc. of the gelatine. The pipette used must be sterilized by being passed through a flame, or it may be well washed out first with distilled water, then with 1-1000 solution of corrosive sublimate, and again with absolute alcohol. Great care must be taken that the plugs do not come in contact with anything outside the tube or flask. To inoculate, heat a platinum wire mounted in glass, and allow it to cool; take some of the material to be inoculated on the point of the wire, hold the mouth of the test-tube downwards, withdraw the cotton-wadding, push the wire up into the gelatine, taking care to withdraw in the same line, re-insert the plug, and label the tube. Various other methods of using the gelatine were then described, for the analysis of air, water, etc.

Koch's Inspissated Blood Serum.—The blood is drawn into tall jars, previously washed with 1-1000 corrosive sublimate and then with alcohol. (The stoppers are greased with vaseline.) It is allowed to stand in the cold for 24 hours, and the serum is removed with a sterilized pipette and run into sterilized test-tubes stoppered with cotton-wadding. These test-tubes are then placed in an upright position in a hot-air chamber, which is maintained at a temperature of from 57° C. to 60° C. for six hours the first two days, and for four hours the following four days. It is now sterilized, but should be quite fluid. To inspissate—Place the test-tubes in a reclining position in a hot-air chamber kept at a temperature of 68° C. to 70° C. (in the case of calves' serum it must be as high as 80° C.) Leave until the serum becomes just solidified. This usually takes place in about twenty minutes, but the serum of the calf requires a somewhat longer time. The serum is then ready for use.

Bread Paste.—Take the crumb of a loaf, break it down or cut into slices, and dry at a temperature of a little more than 40° C.; grate down carefully, and place about 10 grams in a sterilized flask; add 25 cc. distilled water. Heat (in steam) to 100° C. for 30 minutes on two successive days. Allow the paste to stand for at least 48 hours, in order that it may be seen whether any

growth makes its appearance before inoculation. Milk and other fluids may be sterilized in the same manner. Through the kindness of Dr Hare, who added a considerable number of cultivations, a tolerably complete set of typical growths of micro-organisms and fungi was placed before the Society. Several of these (tubercle bacillus, anthrax B., etc.) were brought by Drs Hare and Woodhead from the laboratory of the Gesundheitsamt, Berlin, where, through the generosity of Director Dr Struck, on behalf of the German Government, and the kindness of the officials on the staff, these two gentlemen were enabled to avail themselves of its splendidly organized system. Tubes prepared by Dr Becker, containing micrococci of osteo-myelitis and cultivations from these, were also exhibited.

VII. ORIGINAL COMMUNICATION.

A RARE FORM OF LARYNGEAL NEUROSIS.

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THE affection to which I am about to direct your attention was first described in 1876 by Charcot as laryngeal vertigo. This author had, up to the year 1879, observed four cases; and, so far as I am aware, other writers have contributed to medical literature five more examples of the same neurosis, making in all nine published cases. To these I propose to add a tenth; but before doing so shall endeavour to give a short *résumé* of the clinical histories so far recorded.

Charcot's¹ cases were as follows:—

(1.) A patient (male) who complained of gout and cough, but was certainly not epileptic, was from time to time, after a slight cough, affected by vertigo without loss of consciousness. During these attacks he fell down without a trace of convulsive movement, but very soon recovered and got up. These seizures were first noticed when the cough began.

(2.) Another patient, aged 55 (also a male), not an epileptic, was subject for more than a year to what he called "attacks." These were preceded by tickling in the region below the larynx, and a short, dry cough, which was followed by a sort of fit, during which the patient fell without loss of consciousness,² while the face became turgid, and some convulsive movements occurred in the head and arm. The tongue was never bitten, and

¹ *Revue des Sciences Médicales*, x. 135; *Le Progrès Médical*, 1879, xvii.

² It is right to state here that the account of this case given in the *Revue de Médecine* differs in an important point from that contained in the *Progrès Médical*, where it is stated that the patient lost consciousness.

the attack, which was always short, was scarcely over before the patient got up without hebetude or confusion of intellect. These attacks were frequent (fifteen or sixteen a day); and when the fits of coughing were not followed by such severe paroxysms as above described, the patient only experienced an indescribable feeling of giddiness, without falling. He suffered from granular pharyngitis, bronchitis, and emphysema. The application of nitrate of silver to the mucous membrane of the pharynx, the external use of vesicants over the larynx, and the internal administration of bromide of potassium, effected a cure, in these cases, within a few weeks.

(3.) A medical man, about 40 years of age, after an attack of acute rheumatism, began to suffer from asthma and bronchitis. The paroxysms of the former were often followed by the expectoration of bronchial casts. During the year 1877 he became subject to attacks quite distinct from asthmatic paroxysms. These were preceded by burning and tickling referred to the region below the larynx, followed by a fit of coughing. The patient then experienced giddiness, and almost at the same time the fingers of his left hand became flexed, the left arm became stiff in the position of extension, and was raised almost as high as the head, while the whole limb gave three or four convulsive twitches. The patient then lost consciousness, and when he recovered found himself lying on his left side. At times the premonitory cough was only followed by noisy inspiration and a feeling of suffocation, without further symptoms. Laryngoscopic examination by M. Fauvel revealed only slight hyperæmia of the larynx. Eventually the patient died during a paroxysm of asthma, being up to the last in full possession of consciousness.

(4.) A man, 45 years of age, suffered in July 1878 from bronchitis, which was soon recovered from, but left in the region of the larynx a more or less permanent feeling of tickling and warmth, which were liable to exacerbations producing fits of cough. In August, after one of these paroxysms, he lost consciousness, falling to the ground; and since then such attacks have been frequent, three or four sometimes occurring in the course of a day. During the fits the face became swollen and slightly livid, while convulsive twitching occurred in the face and limbs, but there was no cry, neither was the tongue ever bitten. Immediately after the attack was over the patient got up without a trace of mental confusion. Examination of the larynx gave negative results, while the internal administration of bromide of potassium, with the application of vesicants over the region of the larynx, effected a speedy cure.

Charcot believes that cases of this kind are entitled to be grouped as a distinct clinical variety, and has proposed for them the name of laryngeal vertigo.

In the *Practitioner*, August 1878, Dr Gasquet records the case of a gentleman, 70 years of age, who, after catching cold and

suffering from severe paroxysms of cough, became liable to attacks of loss of consciousness, which were always preceded by laryngeal irritation and coughing. Treatment, directed to the larynx alone in this instance, effected a cure, but, unfortunately, neither the laryngoscopic appearances nor the remedies which were applied are recorded.

In the *Annales des Maladies de l'Oreille et du Larynx*, March 1882, Krishaber relates the following case:—

The patient, X. (male), was lame from an old-standing articular affection, but there was no neuropathic history. At the age of 21 he had pleurisy, followed by complete cure, and auscultation revealed no change in the thoracic organs.

At 25 patient had syphilis, but no tertiary phenomena showed themselves. He married at 32, and had three children, of whom two died in early infancy, while the oldest is of sound constitution. X. was in the enjoyment of good health when, fifteen months ago, he was attacked by loss of consciousness in consequence of violent emotion. That he did not fall is explained by the fact that he was seated at the time. In the course of the same day he had several attacks of dry cough, followed each time by dizziness, but without loss of consciousness. Little by little the cough was disappearing, when about six or eight days from the first attack, on making an effort, he was attacked by a slight cough, followed by loss of consciousness. This time he fell and bruised his forehead. After recovering he felt slightly stunned, but was soon again able to resume his occupation. Another phenomenon meanwhile appeared; the patient suffered, without preceding nausea, from noisy eructations, which were repeated at short intervals, and only ceased in the evening. On the next day another attack was experienced on the street, produced, according to the patient, by a sudden noise, and he again fell down unconscious, but does not remember whether this attack was preceded by cough, like the others. According to those who went to his assistance, unconsciousness lasted a short time only, but the patient became very pale. About two months then passed without any more severe attack than slight cough, followed by eructation and a peculiar, indescribable feeling of malaise, which seemed to resemble a clouding over of his mind (*obnubilation*). The fourth severe attack occurred under peculiarly characteristic circumstances. X. was in a steamer, looking at the bank of the river, when another vessel crossed near that in which he was; under the influence of this surprise the paroxysm originated. On this occasion the patient distinctly remembered that he was immediately attacked by cough and vertigo, which made him lose consciousness. After this the patient wished to get on the bridge, but on attempting to ascend he was again overtaken by cough and giddiness, which obliged him to sit down. He thought that he did not lose consciousness on the second occasion; during the re-

mainder of the day there occurred fits of coughing, which were only followed by slight mental confusion (*étourdissement*), which the patient compares to the sensation experienced after receiving a blow on the head. He is, however, perfectly conscious of the fact that the starting-point of the attacks is the larynx, and instinctively puts his hand to his throat on each occasion of their recurrence.

"On questioning him closely," Krishaber goes on to say, "I learnt that both in slight and severe attacks, followed or not by loss of consciousness, two phenomena commanded attention: the first premonitory symptom was cough, slight and not accompanied by much sound, but frequent, like the last of a paroxysm of whooping-cough, while the second was arrest of all the muscles of respiration. It is evident from the description I have been able to obtain that the suspension of respiration is due not only to spasm of the muscles of the glottis, but to arrested action of the diaphragm and other muscles of respiration."

That if the glottis be closed the muscles of respiration will be greatly hampered in their action is evident; but from a mere history given by a patient it seems to me quite impossible to arrive at the conclusion that their action is arrested. In Krishaber's case, laryngoscopic examination revealed only slight hyperæmia, but, unlike other recorded cases, antispasmodic treatment failed to afford relief.

The next case is that described by Gray (*The American Journal of Neurology and Psychiatry*, Nov. 1882). This author writes:—"In February 1880 there came to consult me a man aged 55, who stated that about a month before he had begun to have a cough, and that when it was severe he would lose consciousness for a short time. There was no history of any convulsive movements whatever. These coughing spells were always preceded by a burning and tickling sensation at about the level of the larynx. A thorough laryngoscopic examination was made by my friend Dr Thomas French, and nothing abnormal was discovered. In 1863 he had been wounded on the head by a bullet, which ploughed up the tissues over the skull from brow to vertex, somewhat to the right of the median line. He was shortly afterwards affected by losses of consciousness similar to the present ones, and they continued for about two months. During the interval he has had colds innumerable, but never any vertigo or unconsciousness. Under treatment with the bromides he had no return of the losses of consciousness, but he was only under my charge about three weeks, and since then I have not seen him."

In the *Archives of Laryngology* (July 1883), Lefferts describes two cases of so-called laryngeal vertigo. To use his own words:—"A gentleman, young, strong, and free—as I have assured myself by careful physical examination—from any abnormality of either heart, lungs, or kidney, is sitting at his dinner-table surrounded

by friends. Suddenly he has a slight attack of spasmodic cough, and a second later falls to the floor unconscious. Almost immediately he arises, resumes his seat and the conversation at the point where it was interrupted. This is not the first attack of this character that my patient has had. Several have occurred during the past eight years; and with much greater frequency—so frequent, indeed, that he has retained no recollection of the number—attacks of partial unconsciousness, always preceded by the same paroxysmal attack of coughing, have occurred. They last but a few seconds, are preceded by blurring of vision, with dizziness or vertigo, and pass away instantly, leaving him clear-headed and bright. I have said that he is a young and strong man, free from organic lesion. He has an incomplete history of hereditary neurosis. There is no evidence of any convulsive movements during his attack. These latter are always ushered in by tickling in the larynx and violent cough; the face becomes suffused. In the worst he falls without cry, but rises immediately, without confusion of ideas, and without remembrance of what has occurred during his brief unconscious interval. In the lighter attacks the premonitory cough is often followed by a few stridulous inspirations and a slight feeling of suffocation, then vertigo and momentary unconsciousness, or it may be slight cough alone, with some spasm of the larynx, slight dizziness, but no unconsciousness. An examination of his larynx shows that, aside from a slight hyperæmia, the appearances are normal; the pharynx is granular, uvula is not elongated.”

Of his second case Lefferts gives only a brief history. “It likewise,” he goes on to say, “concerns a young strong man, and his history would be but a repetition of the first that I have detailed to you, with the exception that his attacks are not so severe; he has had but two unconscious falls, and he does not live in dread of sudden accident in the street or elsewhere. He has no history of other neurosis. A sister is decidedly neurotic. The history of his affection dates back one year only.”

I have thus far endeavoured to bring before you what is known of laryngeal vertigo, as it has been called by Charcot, who is inclined to believe that in this affection we have the analogue of Menière's disease—the afferent nerve being the superior laryngeal. Gray believes that a more accurate designation is to be found in the term laryngeal epilepsy, but bases his arguments on the erroneous assumption that in all previously recorded cases consciousness was completely lost. There can be very little doubt that the term spasm of the glottis of adults, used by Krishaber, is more in accordance with clinical facts. This author draws an analogy between the neurosis in question and so-called nervous or hysterical cough. The latter is, however, most commonly observed in young girls, and more rarely in boys, while the former has so far only been described as occurring in grown men. Moreover, the term employed

by Krishaber would also apply to the stridulous respirations sometimes observed in hysterical females; and for these reasons I think the name complete glottic spasm of adults would be more suitable.

I shall now give briefly the history of a case which has lately come under my care and presents some points of special interest.

D. M., age 35, married, consulted me on the 22nd of October 1883, giving the following history:—Eight months ago he swallowed a fish-bone, which stuck in his throat, and, as the patient thinks, remained there half an hour. Two days afterwards, on going to bed, he experienced difficulty in breathing, and this symptom became so marked that he got up and paced the floor during the whole night. Castor oil was administered, and the throat poulticed, while the patient remained in bed for a week, during the whole of which time—to use his own words—he “felt inclined to be choked.” After this he thought himself able to work, but was obliged to desist owing to attacks of giddiness. While sitting at work (boot-closing) his breathing seemed suddenly to stop, and his head began to swim. This sensation was described by the patient as giddiness, but further questioning soon elicited the fact that in no sense was it a true vertigo. In the latter surrounding objects, or sometimes the sufferer, seem to revolve or rotate; but no such subjective phenomena were experienced by the patient whose case we are considering. During the attacks it was noticed by those in the room that his face became pale, and remained so for some time afterwards. Consciousness was never lost, neither was there any convulsive movement. The fits were preceded by a short cough, and the throat felt as if being “squeezed together;” but there was never any feeling of burning or tickling in the region of the larynx, while—again to use his own words—“all the breathing machinery seemed to stop.” The patient was never aware of any special exciting cause, and stated that the fits came on when he felt quite “at ease.” On one occasion, while standing, he suddenly became pale, and fell back into a chair, so that his wife ran out to get assistance, thinking that death was imminent; but even during this attack the patient retained consciousness.

A peculiar clicking in the throat was complained of, accompanying each act of deglutition, which was referred by the patient to the region of the uvula, and was most probably due to spasmodic action of the palate muscles, more especially those which act on the Eustachian tubes. Examination of the throat showed congestion of the pharynx, the posterior wall of which was granular, and the uvula was drawn to the right. The larynx was congested, and in phonation the slightly thickened vocal cords did not meet as accurately as in health. The whole larynx, including the sinus pyriformis on both sides, was examined, and it can therefore be positively asserted that the symptoms were not due to the presence of a foreign body. Sometimes the patient had difficulty in

swallowing solids. This was, however, only occasional, and often followed over-indulgence in stimulants; for although not a habitual drunkard, he sometimes drank to excess.

On examination of the chest, both lungs and heart were found normal. To verify this fact I sent the patient to Dr James, who concurred with me as to the absence of thoracic disease. Tendon reflex is somewhat exaggerated, and Dr James found slight ankle clonus. The sight is good, and there is no colour-blindness nor unsteadiness when the eyes are closed. There is no history pointing to hereditary neurosis; neither has he ever had syphilis. The giddy attacks recurred about once a fortnight until treatment was begun. The latter consisted in the application of strong astringents to the pharynx, counter-irritation over the larynx, avoidance of alcohol, and the internal administration of the bromides of potassium and ammonium, and the patient has now been for a considerable time without an attack. The diagnosis in this case was simple, for the symptoms could only be due to either locomotor ataxia—which was easily excluded—or to the neurosis under consideration.

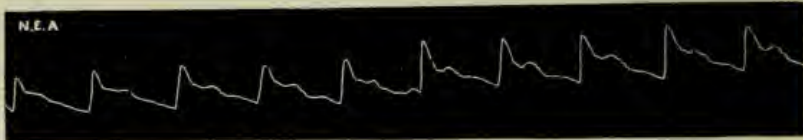
I think the description given by this patient of his attacks fully confirms the view of Krishaber, that the paroxysms are due to spasm of the glottis. Indeed, that this is the case has not, so far as I know, been disputed. Charcot, indeed, seems to believe—as before said—that the attacks are of the nature of giddiness, the irritation being conveyed by the superior laryngeal nerve. The latter is, I need hardly say, the sensory nerve of the larynx, and it is well known that its stimulation produces spasm of the glottis. My patient presented a well-marked tendency to spasm, not only in the region of the larynx, but throughout his whole muscular system. Thus he had exaggerated tendon reflex, ankle clonus, spasmodic action of the palate muscles, and occasionally suffered from spasmodic stricture of the gullet.

Let us now turn to a consideration of the actual chain of events which leads to the paroxysms. In every case so far reported the fit was preceded by a short cough, or, in other words, by a series of spasmodic inspirations, followed by spasmodic expiration, with partially closed glottis. Now, there can be little doubt that the complete spasm of the glottis will replace the act of coughing, and that it therefore occurs just after a full inspiration. To the physical conditions thus produced within the thoracic cavity I believe the attacks to be due.

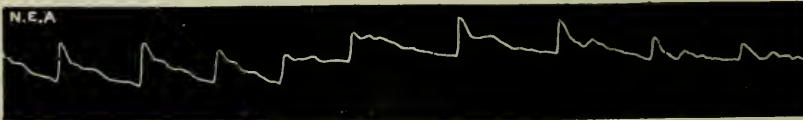
The question next arises as to how these physical conditions can produce dizziness or loss of consciousness. If a healthy person takes a deep inspiration, and then attempts to perform the act of expiration with the glottis closed, it is evident that the thoracic organs are artificially and voluntarily placed under the same conditions which pertain at the beginning of an attack of so-called laryngeal vertigo. The effect upon the circulation produced by

this experiment is very striking, as may be observed by placing the finger on the pulse, which becomes immediately slower and feebler than before. The effect upon the pulse is, however, still better shown by sphygmographic tracings, which, with the kind assistance of Dr Mackay and Mr Aldridge, I have been able to obtain. It will be seen that the result of the experiment is almost complete obliteration of the pulse-tracing; and it is a very interesting fact that the gentleman experimented upon actually experienced a momentary threatening of syncope just after one of the tracings was taken.

I have very little doubt that, in the paroxysms which characterize the disease under consideration, a closure of the glottis occurs im-



Normal Tracing.



Deep inspir. began at 2nd up stroke.
Forced expir. with closed glottis at 5th up stroke.



Deep inspir. followed by forced expir. with closed glottis.

mediately after full inspiration; this is immediately followed by an attempted expiration, and as a result the patient either faints or gets dizzy. Immediately syncope is produced, or, in some cases, just before this stage is reached the spasm relaxes and the attack is over.

To explain exactly how this tendency to syncope is brought about by the physical conditions above considered is a somewhat difficult problem. The increased atmospheric pressure on the walls of the alveoli will in all probability prevent, or tend to prevent, the free passage of blood through the lungs, and therefore lessens the quantity of blood in the left side of the heart. Again, the pressure on the large intra-thoracic veins must hinder the return of venous blood, and thus we can understand that the face will be pale or turgid according as the spasm of the glottis lasts for a longer or

shorter time. It is also quite conceivable that the compression of the heart between the unyielding lungs and the chest wall may help to paralyze its action.

With regard to the nerve channel by which the laryngeal neurosis in question is produced, it is difficult to arrive at any positive conclusion. It is well known that stimulation of the sensory nerves of the pharynx, as well as of the superior laryngeal, will produce spasm of the glottis. In a considerable proportion of the reported cases there was marked pharyngeal catarrh, and in the case observed by me the disease seemed to owe its immediate origin to the presence of a foreign body for a short time in the throat. An interesting fact which I have only just learned is, that my patient has been absolutely free from his attacks ever since treatment was begun, excepting on two occasions, when they followed indulgence in alcohol. On the second of these he was also conscious of a distinct increase of redness and swelling in the throat.

The diagnosis of this affection—if the laryngeal crises of locomotor ataxia be excluded—is easy; but it may be urged that the cases recorded are merely epilepsy with a laryngeal aura. Against this view the most convincing argument seems to me to be the fact that Chareot has spoken of laryngeal vertigo as being quite distinct from epilepsy. Other evidence is to be found in the facts that a considerable proportion of the cases occurred in men of middle or advanced age, that in some consciousness was never lost, that the tongue was never bitten, that convulsive movements rarely occurred, and, finally, that in most of the recorded cases the disease has shown itself very amenable to treatment.

As to the prognosis, the fact that only one death has been recorded, and that in this case it was due to other causes, speaks for itself.

In a very interesting series of clinical lectures on vertigo, Professor Grainger Stewart¹ observes that giddiness “is sometimes produced by violent fits of coughing, sometimes by violent fits of sneezing.” It is then suggested that the immediate cause of the symptom may be changes produced in the middle ear. In view of the effects which I have shown to be produced in the circulation by attempted expiration with closed glottis, it seems more probable that the attacks of giddiness referred to by Dr Stewart are more of the nature of syncope than of auditory vertigo.

While these pages were already in the press, my attention was directed to an interesting series of experiments described by Weber in *Müller's Archiv*² (1851). This author, after pointing out the fact that forced expiration with closed glottis causes weakening and eventually stoppage of the heart's action, gives his views as to

¹ *Edin. Clin. and Path. Journal*, 12th Jan. 1884.

² “Ueber ein Verfahren den Kreislauf des Blutes und die Function des Herzens willkürlich zu unterbrechen.”

the mechanism by which this result is produced, and details graphically the result of carrying the experiment too far, as evidenced in his own person. As Weber's explanation and personal experience go far to corroborate the conclusions which I have independently arrived at, it may be well here to translate two passages from his paper:—"The reason why compression of the chest—even if only by the muscles of expiration—has an effect so marked on the heart and circulation is as follows:—When the thoracic cavity is narrowed by the expiratory muscles, the air contained in the lungs and bronchi, as it cannot escape, is compressed, and by its elasticity exercises a uniform pressure on all the parts within the thorax, *i.e.*, not only on the pulmonary tissue, but also on the heart and large vessels. Now, as the blood contained in the veins only returns to the empty and flaccid heart by virtue of the pressure under which it is placed, it follows that if counter pressure be exercised on the heart and venæ cavæ, as is the case during compression of the intra-thoracic air, the force of the flow must be diminished. If the pressure on the heart be sufficient to balance that of the blood in the veins of the neck and abdomen, or if it exceed it, no more blood can enter the heart or the thoracic veins. The small quantity of blood contained in the thorax, in the venæ cavæ, the heart, or the pulmonary vessels is driven into the aorta by the next contraction, after which no blood can flow from the heart into the aorta. If severe pressure be exercised, the pulse, because the supply of blood to the heart through the venæ cavæ is cut off, immediately becomes small, but continues until the blood contained in the thorax has emptied itself through the left side of the heart into the aorta. Then, usually after three or five beats, the pulse intermits altogether, because no blood reaches the aorta from the empty heart, and only returns after compression of the thorax has ceased."

On one occasion Weber, while experimenting on himself, produced actual syncope, which he thus describes:—"During this interval of unconsciousness slight convulsive twitchings of the face were noticed by the bystanders; and as consciousness returned all recollection of what had taken place was so obliterated that in spite of the fact that my pulse was being counted aloud as before, I could not at first remember where I was and what was happening."

We have in this description the exact counterpart of what occurs in severe cases of the laryngeal neurosis which forms the basis of my paper, and thus, I think, strong support is given to the views expressed in the preceding pages. These facts also show how dangerous to a patient with weak cardiac action is the act of straining, whether during defæcation or in the second stage of labour. The aural surgeon, too, will do well to abstain from recommending Valsalva's experiment (*i.e.*, forced expiration with mouth and nostrils closed) without first ascertaining the condition of the heart.

Meeting IV.—February 6, 1884.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

I. EXHIBITION OF PATIENTS.

1. *Dr Hodsdon* showed a middle-aged man suffering from PARAFFIN ECZEMA, under the care of Dr Duncan, to whom he had been sent by Dr Longmuir of Bathgate. The disease affected both forearms. It seemed to have begun as comedos on the back of the hands. These, gradually enlarging, went on to the formation of horns, as seen on the dorsal surface of the forearms. The horns falling off left a deep ulceration.

Mr Joseph Bell said this case was an addition to the list of paraffin diseases, in which he was much interested. Paraffin acne and epithelioma were not very uncommon among paraffin workers. He had operated on several cases of the latter disease.

2. *Dr Allan Jamieson* showed a young man who presented either a curious condition of a common disease or a combination of two diseases. Three weeks previously he had observed some spots on the chest, which had since spread gradually. When first seen, Dr Jamieson thought the case was one of lichen marginatus, but a microscopic examination of some scrapings showed that these contained the parasite of pityriasis versicolor. The patient also showed a certain amount of psoriasis, best marked below the knees. The question was whether this was simply a development of psoriasis commencing on the back and raising up the epidermis affected by pityriasis versicolor, or whether it was that rare kind of pityriasis versicolor which assumes the circinate form.

II. EXHIBITION OF PATHOLOGICAL SPECIMENS.

Dr Church showed the SUPRA-RENAL CAPSULES and KIDNEYS from his case of Addison's disease.

III. ORIGINAL COMMUNICATIONS.

1. CASE OF MALFORMATION IN A MALE CHILD, IN WHICH THE RECTUM ENDED IN THE MEMBRANOUS PORTION OF THE URETHRA, AND THE FÆCES WERE PASSED THROUGH THE PENIS.

By WILLIAM CRAIG, M.D., F.R.C.S. Ed., F.R.S.E., Lecturer on Materia Medica, Edinburgh School of Medicine.

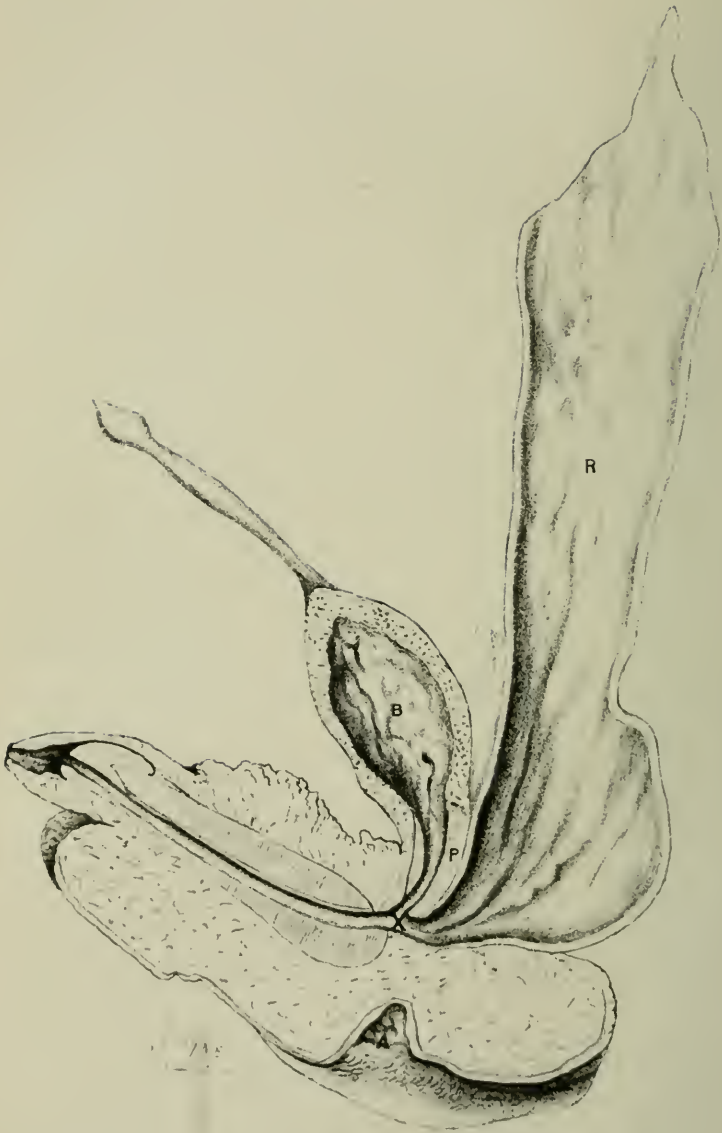
At the first meeting of the present session I showed to the Society the external genitals and pelvic organs of a male child, illustrating the rare malformation of the rectum ending in the

membranous portion of the urethra, and promised to give to the Society, at a future meeting, a history of this rare and interesting case. I shall now implement that promise.

On the 18th of July last I attended one of my patients in her second confinement. The labour was natural, and the child was a strong, healthy male. Nothing abnormal was noticed at the time, either by myself or the nurse. The child was born about 7 A.M. Nothing unusual occurred that day or the next, but on the morning of the 20th, two days after the birth of the child, I was asked to see the child, as it had been crying all night and was passing its fæces through the penis. I at once went and made a careful examination of the parts. I found that the child had a careworn and anxious expression, that the abdomen was somewhat swollen and tender, and on examining the perinæum I found a well-marked raphe and a depression in the region of the anus, but no trace of an anal opening. The external genital organs were well developed.

Being fully convinced that I had a serious malformation to deal with, I at once advised that an experienced surgeon should be called in consultation. After consulting with the father of the infant, Mr Joseph Bell was asked to see the child with me, and on the same morning, about 10 A.M., we met, and, after examining the parts very carefully, Mr Bell made an exploratory incision in the region of the anal depression to the depth of fully an inch, but could discern no trace of the bowel. The wound was kept patent by means of pledgets of lint, but there was no bulging of the bowel. Two days afterwards Mr Bell again saw the patient, and this time Professor Turner was also present and examined the parts. On this occasion Mr Bell extended his incision in the region of the pelvis, but without finding any trace of the rectum. Mr Bell not feeling warranted in extending his incision deeper, the child was left in this state for a few days. During all this time the fæces came regularly by the penis, causing intense agony to the little sufferer. Mr Bell, after several days, again examined the patient, but did not feel justified in interfering further surgically in the case. I examined the wound daily, but could never discover any bulging of the bowel towards the opening made by Mr Bell. The child lingered on till the 5th of August, having lived rather more than eighteen days.

Shortly before the child died I had to leave town for a holiday, but left the child under the care of Dr Mackay, and told him that in the event of the child's death taking place before my return he was at once to communicate with Mr Bell, as I had obtained permission to examine the body after death. As the child died before my return to town, Mr Bell asked Dr Maxwell Ross to perform the post-mortem, which he accordingly did in company with Dr Mackay. I requested that the parts should be preserved in a solution of the hydrate of chloral until my return. The following



R, Rectum ; B, Bladder ; P, Prostate ; ×, Union of Rectum with Urethra.

is Dr Maxwell Ross's account of the autopsy and of the anatomical description of parts:—

“The autopsy was performed some seven or eight hours after death. Leave was given to inspect the abdomen only. Externally the penis, testicles, and scrotum were normal in appearance and development. In the middle line of the perinæum, instead of an anus, there was a depression or *cul-de-sac* of sufficient depth to admit the greater part of the distal phalanx of the little finger. The internal organs were exposed by a crucial incision and inspected *in situ*. Above the pelvis they were normal and healthy. The ascending and transverse colon contained some soft, semi-fluid feculent matter, bright yellow in colour. The descending colon and the pelvic organs, including the penis, scrotum, and testicles, were removed *en masse*, and preserved for some weeks in chloral. They were afterwards transferred to strong spirit for a few days to harden. With the assistance of Mr Symington, a section was made in the middle line of the penis, bladder, and lower part of the rectum. The bladder wall was slightly hypertrophied. There was no communication between the rectum and the bladder, but in front of the prostate gland the rectum entered the membranous portion of the urethra, as shown in the accompanying drawing.

“As can readily be seen by a reference to this drawing, the first and second parts of the rectum were well developed, the latter being somewhat dilated behind the place where it opened into the membranous part of the urethra. The third portion of the rectum was completely undeveloped.”

Such is the anatomical description of the parts by Dr Maxwell Ross, and an interesting question arises, Can this rare malformation be accounted for by a reference to the development of the parts? In cases of simple imperforate anus the case is easily explained by a reference to the mode of development of the anal opening, namely, by “invagination of the outer surface and the opening of a communication between it and the intestine.” True, we have here an imperforate anus, but we have something more; there is the absence of the third part of the rectum, and therefore this explanation cannot hold good in the case before us. I am inclined to the view that in this case there did take place, to a certain extent, at least, this “invagination of the outer surface,” for there is a well-marked depression in the region of the anus; and if the lower portion of the rectum had been fully developed, the probability is that we would not have had even a case of imperforate anus on this occasion. There is evidently a considerable portion of the lower bowel not developed, besides the communication existing between the rectum and urethra. By a reference to the drawing it will be seen that the rectum is prolonged into a narrow tube, which terminates in the membranous portion of the urethra. This can best be explained by a reference to the

development of these parts. At a very early period of embryonic life (about the fourth week) there is "a common opening for the intestine, the generative, and the urinary organs, *i.e.*, a *cloacal* aperture." By the development of tissue a septum is formed between the *sinus uro-genitalis* and the intestine. In the present case this septum was not fully developed, and so we have the remains of a condition, existing in early embryonic life, persistent by this opening between the bowel and urethra. Regarding the non-development of the third portion of the rectum I am unable to offer any explanation.

It thus appears that this rare malformation has been caused partly by the want of development of the tissue which forms the septum between the *sinus uro-genitalis* and the intestine, partly also by the want of development of the lower portion of the rectum, and possibly also to a certain extent by the invagination of tissue which forms the anal opening not having proceeded far enough.

This case is one exceedingly rare. I am not aware of any case exactly similar having occurred. Dr Maxwell Ross has drawn my attention to the fact that Holmes, in his *System of Surgery* (vol. v. page 814, 2nd edition), states that the rectum may communicate with the urinary tract, but relates no cases in detail. He says that "communications between the lower end of the bowel and the male urinary organs, with imperforate anus, constitute the most troublesome variety" of imperforate anus. He states farther that the communication may be either with the urethra or bladder, in which case he expresses the belief that it can be reached from the perinæum.

Dr Maxwell Ross has also drawn my attention to the fact that the *Medical Times and Gazette* of 22nd September 1877 records a case not exactly identical, but presenting features in many respects resembling this one. It is designated, "Case of Imperforate Anus and Fistulous Opening into the Bladder." It is from the *Australian Medical Journal* of March of that year, and is as follows:—"A male child was brought to Dr Rowan for advice (11th April 1876) three days after birth, having an imperforate anus, and the abdomen being very distended and tender. No depression or other sign of the position of the anus existed, and an incision was made at the centre of the fundament large enough to admit the index finger as a guide. On cutting to a depth of about two inches and a half, a fluctuation in the line of the rectum could be felt. On opening this fluctuating mass, a large quantity of meconium and gas escaped, with immediate relief. Oiled lint was passed into the wound, and replaced every morning for about a week, after which a bougie was introduced every second or third day to keep the aperture open. Nothing was heard of the case from May until February 1, 1877, when the passage was found closed, so that a probe could not be

introduced. For two months nothing had passed by this aperture, the motions having come away during that time by the penis until the day before the child's arrival, when the foreskin had become so much closed that it could only pass water with great pain and difficulty. This was remedied by circumcision, and on 6th February an attempt was successfully made to restore the natural passage. Since the second operation no fæces or flatus passed by the penis since two days after the operation."

This case differs in several particulars from the one we are now considering. It was eight months after the birth of the child that fæces first came through the penis, from which we naturally conclude that the opening between the bowel and the bladder was not congenital. In my case there was a well-marked anal depression, but no trace of that in Dr Rowan's case; and I am inclined to the belief that in his case the rectum was more fully developed in its lowest part, which would greatly aid the success of the operation.

I now come to the important question of treatment. In the case of congenital malformations of the internal organs it is impossible to diagnose accurately the true condition of parts during life. This fact must ever be borne in mind when considering the very important question of treatment. It was easy, in this case, to make out that there existed a communication between the lower bowel and the urinary tract, and it was perfectly evident that this communication took place somewhere in front of the bladder, seeing that the urine was passed regularly without any admixture of fæces, but nothing further could be accurately diagnosed.

So far as I am personally concerned, my action was undoubtedly the right one, namely, to call in an experienced surgeon, and to place the case unreservedly in his hands.

Mr Bell adopted a certain course of treatment without saving the child's life, and it is a very important question for consideration and discussion, Was his treatment, unsuccessful as it was, the proper treatment in this case? I have no hesitation in answering that it was, and that the autopsy showed that it was not possible to have saved this child's life.

I think all surgeons will agree in saying that an attempt should be made to reach the bowel through the perinæum. In this case we had a well-marked depression in the region of the anus, and we had a right to expect that if the bowel was to be successfully reached it was by making an incision in this region. On looking at the parts *in situ*, or at the drawing, it is evident that in this case it was possible to pierce the bowel by a trocar and canula, or even by a bistoury; but all surgeons must admit that something more was necessary to ensure success. No operation could be regarded as satisfactory which did not establish a free exit for the fæces through this artificial anus.

This was not simply a case of fistulous opening between the rectum and urethra complicated with imperforate anus. There was, in addition, a considerable deficiency in the development of the rectum. To have enlarged the opening sufficiently to have cut down upon the rectum would have been an operation such as few newly-born children could have survived; and even if the child could have survived such a severe operation, the want of development of the third part of the rectum would have prevented a successful issue of the case. Holmes, in his *Surgery, ut supra*, says, "Should the gut terminate in the urethra, the rectum will only be deficient in its lowest part, and its termination will be accessible from the perinæum. Therefore the proper course is to begin by performing the usual exploratory operation in the perinæum, and, if the rectum be met with, to separate it from its connexions, if possible, and draw it down to the skin." It will be observed that the learned surgeon here uses in the same sentence twice that little but most significant word "if;" and he also adds, "I am not aware, however, of a case in which this has been successfully accomplished." It thus appears that successful operations in such cases as the one now under consideration, by cutting through the perinæum, are either unknown or extremely rare.

If success could not be accomplished by cutting through the perinæum, could not success have been attained by a supra-pubic operation? This operation, at the time, was suggested to me by a young surgeon of this city, who suggested to cut down upon the bowel from the groin, and, having found the rectum, following it downwards till it terminated in the urinary tract, and afterwards to have opened a passage for the fæces through the perinæum. Most surgeons will admit this is an operation which would at once have proved fatal to a child only a few days old, and therefore the only other possible operation was colotomy.

This operation in the newly-born child is not one that will commend itself in the present day. It can scarcely be considered as within the domain of practical surgery. Holmes, in his *System of Surgery*, states that M. Guersant opened the colon eleven times in the groin and once in the loins, without saving one of his patients, and adds, "I have only been able to find a single successful case recorded since the publication of M. Rochard's paper" in 1859, although that publication must have given rise to the performance of many similar operations. "In that case, which occurred under the care of Dr Pooley in the State of New York, the operation was performed on an otherwise healthy male child, in whom there were imperforate rectum and a communication with the bladder. The report was published six months after the operation, the child being then in perfect health. The fæces passed about twice a day from the artificial opening, and the fecal discharge from the bladder appeared to have quite ceased. M. Giraldès, however, had a case of colotomy for imperforate anus, in which the

child lived $2\frac{1}{2}$ months, and died from another cause." It thus appears that the statistics of colotomy are quite against this operation for congenital malformation of the bowel. I have always maintained that even though an artificial anus could be made anywhere above the pubes by means of colotomy, such an operation is not to be commended in the newly-born child. It is one thing to make such an artificial anus in the adult, but quite a different thing in the helpless infant. It was far better that the child should die thus early than merely to have its days prolonged, even for a few years, to a life of great pain and extreme misery.

I have only to add, in conclusion, that the preparation having been divided in the mesial line, I have really now two preparations, or rather two halves of one preparation, each of which illustrates very well this rare and interesting malformation. The one will be presented to the Anatomical Museum of the University of Edinburgh, and the other to the Museum of the Royal College of Surgeons of Edinburgh.

The President said the Society was much indebted to Dr Craig for his communication. They would like to hear the remarks of the surgeon who was connected with the case, and he would therefore call on

Mr Joseph Bell, who pointed out that the admirable description of the case by Dr Craig left him very little to say upon it. He had had some experience of operating for such conditions, more particularly in two classes of cases—first, where the rectum communicated with the vagina in the female child, and secondly, where there was no such communication, the rectum ending in a blind *cul-de-sac*. Operation in the first class of cases was not excessively difficult, and almost uniformly successful. By passing a large doubly-curved bougie into the vagina, then through the opening into the rectal *cul-de-sac*, and pressing the latter downwards, it was generally easy enough to expose the mucous membrane by a careful dissection, open it freely, and stitch the edges to the skin edges made by the cut in the region where the anus ought to be. Much care was, however, required for a long time afterwards to prevent the artificial anus closing. The second class of cases was much more difficult to deal with. Though the operation might be successful so far as reaching the bowel and opening it were concerned, the children themselves did not do well. They rarely reached maturity. They were often badly formed otherwise. His experience was that these malformations were more common in female than in male children. In Dr Craig's case, which was almost unique, he had felt that the want of bulging of the bowel from the absence of meconium made the operation much more difficult. They had no indication where the bowel was; and though at his second examination he had prolonged his incision, making it deeper, he had not felt warranted in going further. Colotomy was con-

sidered, but the child was very feeble, and the parents were against further interference, preferring that it should die than come to maturity with such a deformity.

Professor Simpson expressed the pleasure with which he had listened to Dr Craig's communication. The subject was interesting from its rarity. Mr Bell had rightly stated that these malformations and malformations generally were more common in the female than in the male. He thought Dr Craig's criticism of the jaunty description given by Mr Holmes of operation in such cases was very fair. It must be borne in mind that the organs in the child were not so easily got at as in the adult. In the new-born infant the bladder was not a pelvic organ, and the rectum and bladder in a case of this nature would be at the brim of the pelvis. In this particular case there was also another difficulty, of which Mr Bell had so far reminded them. There was no bulging of the bowel to help to guide the surgeon, as the matter was escaping by the urethra. Most of these children were unsatisfactory from the circumstance that such a malformation was rarely found alone. There was usually some other deficiency, either in the skeleton or the viscera. Several examples of this would be found in works on teratology, such as those of Vrolik, Förster, and Ahlfeld, which he would recommend Dr Craig to consult.

Mr Maxwell Ross said that Dr Craig had kindly asked him to supplement his communication with some additional notes from the literature of this subject. Mr Harrison Cripps had published an exhaustive paper in the *St Bartholomew's Hospital Reports* for 1882, on "Imperforate Rectum and Anus in Children," with a list of one hundred cases collected from different sources. Of these hundred, twenty-five were cases in which the bowel communicated with some part of the genito-urinary tract. Contrary to the experience of Mr Bell and Professor Simpson, the majority, thirteen, occurred in male children. In the twelve female children the communication was with the vagina. Of the thirteen males, nine appeared to have died shortly after birth, two lived over a year, and two were alive when their cases were reported, at the respective ages of three and twelve. In five of the nine cases which died early, the bowel communicated, it is said, with the prostate (prostatic urethra?). In the other four the communication was directly with the bladder. Of the two which lived a year, one was a case reported in the *N. Y. Med. Rec.* by Dr Post. An artificial anus had been made, but closed. The patient lived apparently in good health for a year, though passing fæces by the urethra. A free communication between the rectum and prostate was found at the autopsy. The other case was under the care of Dr Hilles, and was recorded in the *Austral. Med. Journ.* Operation had failed to reach the bowel. The infant continued to pass his fæces through the penis, and was stated to have lived on milk, and to have thriven well. At the end of the year, stoppage having

occurred, he was cut as if for stone, and a quantity of inspissated feces removed from the bladder. He did well for a month, but died of diphtheritic ulceration of the wound. In the remaining two cases operation was successful, and the patients alive at the time of reporting. In both the urine was tinged with meconium at birth. One was operated on by Dr Thorn of Toledo in 1869. Twelve years later the patient was in good health, and the artificial anus doing its duty. The second case occurred in the hospital practice of Mr Howard Marsh. The anus showed a tendency to contract long after operation. It was forcibly dilated when the child was three years old. In Dr Craig's case it was evident that the anal tube, or third part of the rectum, had not developed, and that the septum between the bowel and genito-urinary tract had not completely formed. It was interesting to note that in two of the cases collected by Mr Cripps (one published by Tarnier in the *Bull. Soc. Chir. Paris* for 1873, the other in the same journal for 1875, by Polaillon) the other end of the alimentary canal, as well as the rectal portion, was incompletely developed. In Tarnier's case there was a fissure between the cesophagus and the trachea. The child died of asphyxia. In Polaillon's the cesophagus ended in a *cul-de-sac*, the trachea communicating with the stomach. Mr Cripps's opinion regarding the treatment of these malformations agreed with the opinions expressed by Mr Holmes and Mr Bell, that they were very unsatisfactory and but little amenable to surgical interference.

Dr Craig thanked the Society for the kind manner in which his paper had been received. He was of opinion that success or the want of it in the treatment of such malformations depended greatly on the extent to which the rectum was developed. Had the third part of the rectum been developed in his case, he had no doubt that Mr Bell would have succeeded in his operation. This was a point very often lost sight of in recording these cases.

2. CLINICAL OBSERVATIONS ON A CASE OF PERFORATING GASTRIC ULCER.

By J. O. AFFLECK, M.D., F.R.C.P. Ed., Senior Assistant Physician to the Royal Infirmary, Edinburgh, Lecturer on Practice of Physic, Extra-Mural Medical School, Edinburgh.

JOHN M., æt. 21, police constable, was brought into the Royal Infirmary about 7 o'clock on the morning of the 4th May last, suffering from violent pain in the abdomen, with which he had been seized while on duty. So severe was the pain that he had to be brought to the hospital on a stretcher. He was attended to by the house physician on duty, and was admitted into Ward VI. under my care.

State on Admission.—The patient was a man of about 6 feet in

height, and of very powerful physique. He was in great agony from pain in the belly, which had seized him suddenly, and of which the following brief account was obtained. He had gone on duty the previous night at the usual hour, in his ordinary health, with the exception of a slight cold which he had had for some days. During the night, while going his rounds, he had complained to his sergeant of some internal pain, and was advised to go home, but he himself thought he would be quite able to hold on until his time for being relieved in the morning. He had a cup of coffee in the course of the night. About 6 in the morning, while in the act of coughing, he was seized with the violent pain for which he was brought to the Infirmary. On being put to bed the patient rolled about in agony and then lay on his face. He vomited from time to time some brown coloured bilious matter. The abdomen was tender all over, and no proper examination could be made. His pulse was 90, his temperature normal, his tongue slightly furred and coated with sticky mucus. He had an anxious suffering look, but was quite conscious. Hot fomentations were applied to the abdomen, various palliatives employed, and an enema administered, but without result. When I saw him about a quarter before twelve, he was lying on his back with his legs drawn up, in a semi-collapsed condition, with a rapid, feeble pulse, quick shallow breathing of markedly thoracic character, complaining of great abdominal pain and tenderness, so that palpation could scarcely be attempted. The pain was greatest in the upper part of the abdominal region. There was no evidence of hernia nor any localized swelling of the abdomen, which on the contrary was decidedly retracted all over. The heart's sounds were natural, but there were some dry bronchial râles in the chest. I regarded the case as one of acute peritonitis, but the suddenness of the onset and the severity of the symptoms suggested some antecedent lesion as giving rise to the inflammation—such as perforation or strangulation of the bowel. I ordered a powder of one grain of opium and five grains of bismuth to be given every two hours, another enema, as the bowels had been constipated, and turpentine stupes to the abdomen. Small quantities of liquid nourishment were also given at intervals. After the patient took the first powder the vomiting entirely ceased. I came back to the Infirmary in the evening about 7, and found the patient half sitting up in bed, deeply livid or leaden coloured all over, pulseless, and sinking rapidly, but still quite conscious. He died about 9—fourteen hours after admission.

Post-mortem Examination of the Abdomen only was allowed.—It was made 17 hours after death, by the pathologist. On opening the abdomen there was at once observed evidence of extensive peritonitis in the form of recent lymph gluing the folds of intestine together. The abdominal cavity was found to contain a large quantity of yellow, sour-smelling fluid in which were floating white coagula, evidently portions of curdled milk, thus indicating a per-

foration probably of the stomach. This proved to be the case, as on raising the organ an opening fully larger than a sixpence, apparently that of an ulcer, was found in the greater curvature, on the posterior wall, and near the cardiac end. On removing and cutting open the stomach, the ulcer proved to be a marked example of the perforating gastric ulcer, nearly an inch in diameter, round in form, with well-defined somewhat thickened edges. It was manifestly one of the chronic variety, and had eroded all the coats of the stomach, the peritoneal coat doubtless giving way and opening by a large rent when the patient had the attack of coughing in the morning. The stomach otherwise was remarkably free from morbid appearance, and the other abdominal viscera were healthy.

This case presents some features of interest that appear to be not unworthy of notice.

In the first place, it is an instance of the somewhat rare occurrence of perforating gastric ulcer in a man. It is well known that the disease is at least twice more common in women. But it illustrates in addition the fact, which has been oftentimes observed, but which cannot be too constantly borne in mind, that this serious and often fatal disease may exist in a marked form in a person possessing to all appearance a high degree of health. Doubtless such an important lesion as a gastric ulcer can scarcely ever exist without producing some symptoms, and probably strict inquiry might have furnished information of occasional pain, or more or less dyspepsia; but in the present instance these certainly did not attain to any magnitude, and indeed, as far as could be ascertained, there did not seem to have been any manifestation of the disease during life. On the contrary, the patient was regarded as a very strong and fine specimen of a young constable. It is, however, in such very instances of the disease, where no distinct symptoms betray its existence, that this fatal occurrence is apt to take place, and for obvious reasons. The patient has not the advantage of being under the treatment, both hygienic and dietetic, which would go far to avert such a catastrophe. He takes his ordinary food and does his usual work, when, some day, after a full meal or under a heavy strain, the perforation takes place. It has occurred to me to observe more than one instance of this kind, and such cases convey the admonition that where symptoms at all pointing to gastric ulcer exist they ought to be carefully investigated, and the utmost caution be enjoined as to the diet and mode of life. The healthy character of the gastric mucous membrane generally in this case is a point of much interest. Similar appearances have often been observed in this disease. Dr Matthew Baillie, in his work on *Pathological Anatomy*, refers to this, and in several instructive clinical cases narrated by Dr Abercrombie in his work on *Diseases of the Stomach* this striking appearance is noticed. This fact seems to lend strength to the view that some limited nutritive change, *e.g.*, thrombosis or embolism, or

areas of lowered vitality, upon which local irritations or a hyperacid gastric juice exercise their effects, precedes the development of the ulcer. But, again, the locality of the ulcer in this case was that of the least frequent site of the lesion, namely, the greater curvature and the cardiac end of the stomach. Thus, according to Dr Brinton's statistics, only 5 out of 220 were in the greater curvature, and the same is the proportion of the cases of perforation in relation to locality. And, further, the age of the patient differed considerably from the average age of perforations in the case of men. Dr Brinton, in his classical work on the subject, points out that while the liability to perforation during the whole of life is nearly equal in the two sexes, the distribution of this liability at various periods of life varies materially, and he gives as the average age of the subject of perforation—27 in the female and 42 in the male.

The symptoms in the case pointed clearly to some serious visceral lesion within the abdomen, and suggested the occurrence of sudden perforation or strangulation of the bowel, for it scarcely ever happens that acute idiopathic peritonitis is attended with such urgent manifestations at the outset. A certain diagnosis was scarcely possible in the circumstances, since there was no history of previous illness. In all such sudden and violent seizures one of the first points is to endeavour to ascertain the nature of the lesion, and the physician has always to bear in mind the possibility of the existence of a strangulated hernia, or of some other form of obstruction of the bowel, which may make itself evident to palpation. Even, however, in such instances death seldom takes place for two or three days, by which time the obstruction is made plain by the occurrence of fecal vomiting, etc. The suddenness of the symptoms of sinking is of considerable diagnostic importance. Death from perforation does not seem to be produced entirely or immediately by peritonitis, but rather it would appear—at least in no small measure—by the shock which perforation entails. Cases have been recorded where death has followed the perforation almost instantaneously. On the other hand, of course, instances occur where the fatal result in undoubted perforation is delayed for several days; while, again, a few remarkable examples have been observed of recovery following symptoms of this accident. But I believe the general experience is that death takes place after perforation in from twelve to twenty-four hours; and the influence of the shock is shown by the rapid collapse of strength, the small flickering pulse, the quick, shallow breathing, and the remarkable leaden hue the patient assumes before the termination—due, doubtless, to general venous stasis in connexion with the somewhat sudden failure of the heart, which might be characterized as a condition of acute asthenia. Whether the perforation be of the stomach or intestine the symptoms are very much alike, as I have now and then occasion to observe in the fever wards of the Infir-

mary in the case of the giving way of typhoid ulcers. There is probably, however, in the latter case, on the whole, less suffering than in such an instance as that of J. M.

In dealing with the formidable symptoms of perforation there are certain indications which are important to follow—one of which is the relief of the pain. Probably, in most instances, the hypodermic injection of morphia will be most serviceable, but if an opiate be given by the mouth, the best form for administration would be the powder. Feeding is best done by the bowel, although the patient often suffers from thirst for which he desires fluids to drink. In cases like the present, where no previous history exists to throw light on the seat of the lesion, and where yet the occurrence of perforation is rendered probable by the symptoms, it would, perhaps, be the most judicious course to act as if the stomach were the organ perforated, and to avoid as far as possible the administration by the mouth of either food or medicine. For, unquestionably, should perforation of the stomach exist, the passage of the ingested matters through the opening into the peritoneal cavity will only have the effect of rendering more general the succeeding peritonitis, and thus extinguishing any slight chance the patient might otherwise have.

The President remarked that this was a most interesting case, and the clinical observations of Dr Affleck were exceedingly *apropos*. The patient, who was in the police force, was well known to him, but had never applied for medical advice. When he saw him lying on the table in the pathological theatre he was very much struck with his splendid physical development, presenting a very striking contrast to the leuco-phlegmatic appearance of the women who so often suffered from this disease. He was sure the Society would be pleased to hear further remarks on the case.

Dr Shand considered that it was his moral duty, as an old country practitioner, to give information when he could. He accordingly gave details of two cases of sudden death from perforation of a gastric ulcer occurring in women, one aged 45, the other 30. The first of these told him, some time before her last illness, that she had got more good from the old *pil. ferri co.* of the London Pharmacopœia than from any other remedy.

Dr Byrom Bramwell said that Dr Affleck had so ably brought before them the particulars of this case that he had left very little to be said. There was, however, one point to which he thought attention might be drawn, viz., the medico-legal interest of such cases. When people died suddenly, as this man had done, the idea of irritant poisoning was very apt to occur. He had once been called upon to examine the body of a servant girl in Newcastle who died within twelve hours from the commencement of the symptoms. The coroner had been communicated with, under the idea that the girl had been poisoned. The body was extremely

well nourished, and, so far as could be ascertained, the patient had not complained of any stomach symptoms before the onset of the attack, which commenced with acute pain in the region of the stomach, and vomiting. The post-mortem showed ulceration at the pyloric end of the stomach. It was very difficult, in cases of sudden acute peritonitis suddenly arising in perfectly healthy persons, to come to a conclusion as to the exact pathological cause of the symptoms. In addition to a perforating ulcer of the stomach, which was certainly the most common cause of this sudden acute peritonitis, the rupture of a hydatid cyst, or of an abscess which had been latent for some time, were, in his experience, the next most common causes in men. As Dr Affleck had pointed out, a sudden volvulus, which sometimes causes a similar condition, was more easily diagnosed. In women the rupture of an extra-uterine cyst was a not unfrequent cause of immediate peritonitis, and was sometimes mistaken for irritant poisoning. He had a specimen of this kind from the body of a young lady who had been dancing at a ball, and, after partaking of some refreshment, was seized with acute pain in the abdomen, vomiting, and diarrhoea. She died in a few hours of acute peritonitis. On opening the abdomen the case was seen to be one of extra-uterine cyst, a Fallopian gestation about three months, which had ruptured into the peritoneum.

Dr Carmichael was also interested in the medico-legal aspect of these cases. Many years ago he had met with a case of very sudden death in a young married woman who was on unfriendly relations with her husband. The post-mortem showed similar appearances to those described by Dr Affleck, the ulcer being on the posterior surface of the stomach, but nearer to the pyloric end. The patient had apparently been in perfect health, not having required any medical treatment previously. The procurator-fiscal asked him to send in a report, and after the post-mortem examination he had no difficulty in giving it as his opinion that the patient died from natural causes.

Professor Simpson said that as Dr Bramwell was giving a category of the more common causes, in his experience, of attacks of sudden acute peritonitis, his mind was recalling instances of torsion of an ovarian tumour, bursting of an ovarian cyst, of a cancerous uterus, and of a pelvic abscess, as causing the same condition. Last summer he saw in Fife a lady under the care of two medical men, who thought she was dying of an escape of the contents from some portion of the gastro-intestinal canal into the peritoneum. On the morning of the day he was sent for, she was suddenly seized with severe pain, followed by collapse. The practitioners—in whose minds the case of a young man in the neighbourhood who had died shortly before of perforation of a gastric ulcer was uppermost—thought that the lady was suffering from the same lesion. On a little careful inquiry the Professor

came to the conclusion that there might have been conception, because he found out that for two months there had been amenorrhœa. The prognosis varied according to the diagnosis. Was the idea of a perforation right, the case was hopeless. Had they to do with a burst extra-uterine sac, there was some faint hope of recovery. A pelvic examination showed that blood was filling the pelvic cavity, but that might be from some abdominal lesion. All the same the impression he got from the examination was that it was a case of extra-uterine gestation. The patient ultimately rallied, and was now well. He had had no opportunity since of confirming his diagnosis, but he had no doubt it was the correct one. One point in the case had not been touched on, the question of surgical interference, on which he should like to have Mr Bell's opinion. It was a grave question whether a patient with a thready pulse should be subjected to such an operation as opening the abdomen; but one gathered from the notes of Dr Affleck's interesting case that, had a surgeon been present and opened the abdomen, he might have found the conditions observed at the post-mortem and perhaps repaired them.

Mr Bell said he could not help feeling that if he were in this young man's position he should like to have an operation tried, a laparotomy to find out and put right, if possible, the cause of the symptoms. The operation was possible. It had been done in a few cases, and in one or two had been successful. The case reminded him of one that had been brought before the Society by his father and the late Dr Warburton Begbie. It was that of a young gentleman who had been running in the game of hare and hounds over the Pentlands, and had reached Roslin, where, with one or two others, he sat down in the inn to a dinner of roast-beef and mustard, with potatoes, and a small glass of beer. He then went out on the bridge, felt a sudden pain in the abdomen, and had to be carried home to Merchiston Castle. The post-mortem showed that a perforation of the stomach had occurred, and pieces of roast-beef were found in the peritoneal cavity. He was a young man of very fine physique, and had never before shown symptoms of a gastric ulcer.

Dr Affleck said surgical interference in this case was out of the question. The patient was in a state of utter collapse, and could not have borne an operation of such gravity. The medico-legal aspect of the case was before his mind, though he had not referred to it. Indeed, he thought, when he saw the case in his ward (VI.), which was reserved for patients suffering from acute nervous maladies, poisoning, etc., that it had been put there under the supposition that it was a case of irritant poisoning.

3. A CASE OF ADDISON'S DISEASE AT AN UNUSUALLY EARLY AGE, WITH DESCRIPTION OF MICROSCOPIC SECTIONS OF THE SUPRA-RENAL CAPSULES.

By HENRY M. CHURCH, M.D., F.R.C.P. Ed.

As Addison's disease is almost entirely confined to the laborious period of life and exceedingly rare in children, the record of this case is called for. Merkel (in his article in *Zeimssen's Encyclopaedia*) writes, "the disease occurs most frequently in the prime of life, from the age of 15 to 45, no instance having as yet been recorded as occurring before 10 or after 60." In the following case the disease commenced about the age of 9½ years.

M. W., a little girl, aged 10, came under my care in August last, suffering from Addison's disease. Five years ago she suffered from an affection of the lungs, from which she seemed to have recovered, though never became so strong as formerly. Family history was good, with the exception of a maternal uncle, who died of phthisis pulmonalis. There is no history of Addison's disease in any member of the family, though an interesting point regarding her mother is that, though healthy, she has for many years had well-marked bronzing (cloasma) of the forehead and temples. The other children in the family are strong and healthy.

This little girl's illness began about twelve months before I saw her, with no other symptoms than slight discoloration of the face, which the friends ascribed to jaundice. She continued at school all the year till a month before her death, and was able to walk a mile to and from. At school, however, she was listless and made little progress. During this time she complained of constant headache and pain in the side if her companions pushed her about. Her mother noticed that her appetite became poor, and that her skin gradually darkened not only on the face but over the body.

When called to see her I found that the usual symptoms of advanced Addison's disease had come on somewhat suddenly, and that they were well marked. She was unable to rise on account of weakness, breathlessness, and giddiness, and suffered from distressing sickness and retching. Her skin was of a dark bronze colour, her eyes glistening, the whites of the sclerotics being particularly distinct. The pulse was rapid and weak, the skin moist and clammy, the extremities cold. Bowels and kidneys acted normally. She continued in this state for a week and died in a convulsion.

For some days before her death an intensely disagreeable odour emanated from her body, which made it difficult to remain by the bedside.

On post-mortem examination, made twenty-four hours after death, this fœtid odour had quite disappeared. The skin was of a

dusky colour, darker on the face, in the axillæ, on the mammæ and umbilicus. The mucous membranes were also bronzed. The left lung at its apex had cheesy nodules and the remains of a small cavity. The lung was closely adherent to the costal pleura. The right lung was healthy, with the exception of a calcareous nodule the size of a pin's head at its apex. The heart was remarkably small, with fatty degeneration of its muscular tissue. In the right ventricle there was a colourless clot, and in the left a considerable quantity of blood. The peritoneum presented a dingy appearance. There was a localized peritonitis about the bowel pulled out from the neighbourhood of the left kidney. The kidneys themselves seemed normal, but both supra-renal capsules enlarged; the right, about one-eighth the size of the kidney, is not adherent, and is separated from it by a layer of fat. On making a section of it a calcareous body dropped out, possibly indicating, as Dr Greenhow suggests, a more advanced degree of supra-renal disease. The left capsule is about the one-third of the size of the kidney. It is intimately adherent to the kidney, but the substance of the supra-renal tumour does not pass into the kidney tissue. On section this capsule presents a uniform homogeneous opaque appearance except at its upper angle, where there is a fawn-coloured spot about the size of a split pea, which upon microscopic examination seems to be the residue of the normal structure of the supra-renal capsule. The colouring has been due to a hæmorrhage.

Dr R. W. Philip, assistant to the Professor of the Practice of Physic, has kindly prepared for me four microscopic sections, which show the pathology of the supra-renal capsules in this case.

1. A section of the fawn-coloured area referred to, seen under a low power of 50 diameters, presents a reticulated structure, with granular matter in its alveoli. This reticulated structure is pressed on on all sides, so that the alveoli gradually become less and less distinct towards the periphery until the spaces of the network are lost, and the section presents the appearance of a fibrous tissue. On one side of the meshwork the encroaching mass has less of a fibrous character, but is more distinctly cellular.

2. Under a high power of 300 diameters this section presents a reticulated appearance, in the interspaces of which lie abundant cells of small size, though not of equal size. The cells are irregularly round, while some are distinctly hexagonal. Probably these cells are the altered blood corpuscles of a hæmorrhage which had taken place into the substance of the capsule.

3. In a section through the junction of the fawn-coloured normal area and the lighter part of the surrounding structure, it is seen that the walls of fibrous tissue forming the alveoli become more compacted, and the cells in the alveoli less distinct. The section presents the usual appearance of close fibrous tissue with

connective tissue corpuscles more or less abundantly distributed in its meshes. At some points the cell elements are a conspicuous feature, the fibres being comparatively few.

4. As seen under a high power, a section through the general mass of the capsule, apart from the fawn-coloured area, shows in general fibrous stroma with round cells and well-marked nuclei, and scattered in different parts of the field large irregular cells, with branching processes passing out to the general stroma. These latter are true giant cells which, with their several large well-marked nuclei, form the principal features in the field of the microscope.

The President asked if the child had sustained any injury.

Dr Church replied that there was no history of that.

Dr Webster thought the size of the supra-renal capsules in this case very remarkable. The bronzing of the mucous membranes was exceedingly marked. The larynx, which he had abstracted, was as much bronzed as the skin itself.

Dr Byrom Bramwell asked if the condition of the nerves in the neighbourhood of the capsules had been noted. This was a most important point, because it had been shown that the branches of the sympathetic nerve ramifying over and round the capsules were inflamed, thickened, and enlarged in this disease. It was a curious fact that only tubercular disease of the capsules produced this bronzing. Large tumours of the gland were sometimes met with, but they did not cause bronzing or discoloration of the skin. *Dr Bramwell* had repeatedly seen both capsules completely destroyed by cancer, two cases of this description having come under his observation within the past year, without any bronzing. The mere destruction of the capsules did not, therefore, produce the condition. It was an important and interesting question why they got bronzing in tubercular disease only. One would suppose that a cancerous change would set up as much irritation as a tubercular. Unless we believed that the irritation was much more intense, or that there was a specific irritation of the nerves by the tubercular matter, it was difficult to understand. They sometimes got bronzing in cases of phthisis in which the capsules were not affected at all. He had known cases mistaken for Addison's disease in which there were tubercular disease of the lung and bronzing of the skin. It might be a question whether the tubercle bacillus or some other product of the condition did not act as a special form of irritant to the branches of the sympathetic, and so lead to the bronzing.

Dr Church regretted that the nerves had not been examined. There was no adhesion of the capsules to the surrounding structures, except on the left side, where the capsule was adherent to the kidney.

Extraordinary Meeting.—February 27, 1884.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

CATHETER FEVER.

By Sir ANDREW CLARK, Bart., M.D., LL.D., F.R.C.P., &c., Lecturer on Clinical Medicine, and Physician, London Hospital, &c., &c., London.

THE *President* (Dr Littlejohn), in calling on Sir Andrew Clark to deliver his address on Catheter Fever, said: Gentlemen,—You all know the object of our meeting to-night. It is a special meeting of the Society to hear a paper by Sir Andrew Clark, and I am happy to inform you that he has a perfect right to come to Edinburgh at any time and read a paper here, because he is one of our corresponding members. I am sure you will give a hearty welcome to Sir Andrew Clark. I will not take up your time further, but will call on him to read the paper which he has kindly agreed to do.

Sir Andrew Clark said: Mr President and Gentlemen,—In rising to address you upon this occasion, there come between me and the immediate object of our meeting the recollections of my own academic youth. They force themselves upon my mind, and they stir up therein emotions which struggle for utterance, and which cannot, and perhaps ought not to be wholly repressed. For the better part of a third of a century I have been a practitioner of medicine; and when, standing here, I look back over those long years of difficult, anxious, responsible, and sometimes even perilous toil, I see with a clearer, a fuller, and a juster view than ever before the magnitude of my indebtedness to the great medical school of the ancient city in which to-night we are assembled. Those who know me and labour with me in the daily work of my life know, alas! much better than I can know, the imperfections, the failures, the errors, which have marked it; and in all such respects I can say with unaffected sincerity that no one can think of me more humbly than I can think of myself. On the other hand, no one can know, as I know, in struggling to make that work better than it is, the spirit by which I have been animated and sustained, the examples which I have striven to follow, and the principles which have regulated all my work in the exercise of the common art which we practise. Here, in those early years, I found the example, the spirit, the precept. Here, when I was a student, the very atmosphere was penetrated by an earnest, a serious, and a high purpose; and you could no more dwell in the midst of it and remain uninfluenced by it than you could pass through a garden and not carry away with you something of the scent of the flowers. Here, in the persons of Goodsir, Thomson, Syme, and Alison, I had the examples of simple,

self-denying, useful, noble lives, engaged in getting and giving knowledge—diminishing the sum of human suffering, and adding to the sum of human happiness, and finding their own exceeding great reward therein. Here I learned that, at least, chronic disease was the mere penalty paid for the violation of natural laws, that pathological are but mere physiological processes in new conditions; that local maladies are to be interpreted in the light of constitutional states; that in illness the patient rather than the disease is to be the subject of treatment, and that when life is imperilled you are to discover and, if possible, to obviate the tendency to death. Such and many other fundamental principles of our art I learned here and here only. But, sir, education has other claims than those which are merely intellectual and technical. It has claims of conduct and character which are the foundations and the abiding strength of life. Here, too, then, I learned the true force of will, the priceless worth of self-control, the strength—begotten of self-dependence—and the inexpressible satisfactions of thorough work. Here, above all things—may I be forgiven for alluding to such things—here I learned from my revered master, Alison, the great physician and wise philanthropist, that although there may be justly differences between man and man about some of the great questions of life, there is one of them about which there can be no question at all—that the life of this life is righteousness—the subdual and the setting aside of one's self for what is true, and pure, and just, and good, and merciful. Many precious things did this great master teach to me; but this, alas! he could not teach. He could only set it forth and show it in his life and work. For from him also I learned that righteousness is incommunicable; that the secret of its sweetness and strength, its peace and joy, the secret of its beneficence and blessedness dwells entirely and dwells exclusively with them who truly follow after it. Sir, I rejoice that this occasion has arisen when I may make to you and to this assembly—the successors and representatives of those illustrious men of whom I have spoken—this heartfelt but most inadequate expression of the debt which I owe to them. Too great either to be repaid or to be forgotten, I must for ever be content to remain their grateful debtor, and for ever find, in the remembrance of my indebtedness, an abiding reason for striving to make myself worthier of it than I am.

And now, sir, to the immediate object of our meeting. The subject which is to be discussed is what, for the moment, has been called Catheter Fever. It may be asked, and indeed it has been already asked, what right a physician has to deal with a subject which turns so much more on surgery than on medicine. I answer, that the right lies in the duty which the physician in such cases is often called upon to discharge. The patient has been subjected to a surgical operation. The surgical operation

has apparently ceased to trouble the patient, but constitutional symptoms of a grave kind have arisen, and the patient falls into peril of death. And then sometimes the surgeon—more often the friends—wish further counsel, and the counsel which naturally, and I trust I may be pardoned for saying excusably, they seek is the counsel of the physician; for they think, and probably they think rightly, that there are two sides to such a case, and that the one side which may not be covered by the surgeon may be covered by the physician, and that by a careful examination of both a complete study of the patient may be obtained. I have been on many occasions called in to cases of this kind, and the conclusion has forced itself on my mind that the subject of which I am about to speak has not as yet—or, at least, till lately—had any adequate, I do not say any place, but any adequate place in surgical literature or surgical teaching. It seems to me that till lately our knowledge of the subject was neither sufficiently clear nor sufficiently correct; and seeing that such knowledge as we have on the subject was not, as I thought, widely enough diffused, and that its diffusion was of supreme importance to all of us, I have before asked for information, as I am about to ask for it again to-night. It may be that my mode of asking for information on this topic has been unfortunate, but at all events I will venture to say for myself that it has been done in singleness of purpose. I find myself, however, on this occasion as over forty years ago I found myself on a like occasion. It was my duty forty years ago to call weekly upon a lady. She was single, careful, quaint, and anxious to have the credit without the expense of hospitality. At the close of my visit it was the lady's habit to say, "Noo, Maister Andrew, will you hae a glass o' wine or will you raither no?" I had been duly told and well understood that the proper answer to be returned to this inquiry was "*rather no.*" On one occasion, however, I was wicked enough to accept the offer implied by the question. But instead of getting the wine to gladden my heart, I got showers of insinuations and reproaches to punish me for my wickedness. Now, in reference to this catheter fever, a like result has happened. In a paper upon this subject, written by one of the latest and ablest of our surgical workers, he says, with an insinuation of reproach, that physicians take little or no interest in the matter. But when on a late occasion I, as a physician, proceeded to prove my interest in urinary fever, and to ask for further information concerning it, the surgeons, like my quaint old lady, fell vigorously upon me, and by means of denials and accusations, reproofs and warnings, they at once resented and punished my interference.

Now, I think it is almost a truism to say that the introduction of a catheter into the urethra, or any like operation, is occasionally followed by constitutional disturbance—on one occasion slight, and on another occasion severe, which sometimes ends in recovery and

sometimes ends in death. Now, it is alleged, and this is the point of importance—that in all such cases ending fatally, death is due to renal disease which had existed before the use of the catheter, and which in the end leads to uræmia. In fact, this is the crucial statement of the whole discussion which has taken place on this question. May I repeat it? First, it is admitted that the introduction of the catheter is occasionally followed by severe constitutional disturbance, that occasionally this disturbance ends in recovery, but that sometimes it ends in death; and then—and here, I repeat, is the crucial question of the whole discussion—it is alleged that if it ends in death, renal disease existed before the catheter was introduced, and that death is caused by uræmia, in consequence of extension of the renal disease. Now, I demur to this conclusion, and I traverse its accuracy. I contend that in persons apparently healthy catheterism is sometimes followed by fever, neither distinctly uræmic nor distinctly pyæmic; that this fever sometimes ends in death, and that in *such* cases the autopsy reveals no definite, visible, structural lesion adequate to account for death. I have used two terms here which I must explain. I have used the term “apparently healthy,” and also “persons being in health but with a low density of urine.” I have said “apparently healthy,” and this phrase has been taken exception to, but I am bound to resist the exception. I contend that if a man is brought before me whose functions, as far as I can ascertain them by modern means of research, are sound, who eats and digests well, whose urine is of normal constitution, who has no subjective nor objective signs of disease, I am bound to say that such a man is apparently healthy, and any official in a life assurance office—provided that the family was healthy—would accept such a life as good. But the exception is to my using the word “apparently.” I cannot say positively that *any* man is healthy, but I can say that he is “apparently healthy;” and what I understand by that is, that so far as any means that we possess for detecting disease can help me, there is in the case in question no evidence of disease existing or threatened. Then I have said “with a low density of urine.” Thirty years ago I stated in the first volume of the London Hospital Reports, that it was not necessary to have albumen or casts in the urine to indicate renal disease; but that, if a man habitually passed a low density without an increase of quantity of urine, it was quite sufficient to infer from that fact alone that his kidneys were not sound. A larger experience requires this statement to be modified. I should like to put in place of it that a man, in other respects healthy, and passing habitually only an average quantity of urine of a lower density than 1012, has not, in the proper sense of the term, *diseased*, but *inadequate* kidneys. He has what I have called renal inadequacy. A man may have a skin which does not produce a proper sweat, and yet, if you were to examine that skin by every available means for the presence or absence of disease

Before Interference.

Pathogenic Hypothesis on Nature of Troubles following.

Type of Illness.

Essential Clinical Features.

Result.

Average Duration.

Authorities.

| Before Interference. | Pathogenic Hypothesis on Nature of Troubles following. | Type of Illness. | Essential Clinical Features. | Result. | Average Duration. | Authorities. | | | | |
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| Healthy. | Sudden Nervous Disorder. | Shock. | Simple (Apyretic Rigors). Faintness—Epileptiform Seizures. | Recovery. | { Few minutes to few hours. | Nunn, <i>Lancet</i> , 1871, li, 909. Erichsen (<i>M. Beck</i>), <i>Surgery</i> , 7th ed., li, 746. Sir H. Thompson, <i>B. M. J.</i> , 1883, li, 1238. Savory, <i>B. M. J.</i> , 1883, li, 1246. H. Cameron, <i>Glasg. M. J.</i> , March 1884. | | | | |
| | | | Acute Single or Recurrent febrile paroxysms (Rigors). | Recovery. Collapse, Death. | { Few hours to two or three days. | | | | | |
| | Slow Nervous Disorder. | General Dystrophy. | Irritative Fever. | { Low irregular remittent fever (Sub-intermittent fever). | Recovery. Death. Brodie. | { Two or three days to few weeks. | Velpau, <i>Leçons orales de clin. chir.</i> , lii, p. 324 etc. Civiale, <i>Traité pratique sur les maladies des organes génito-urinaires</i> , Perdrigou, <i>Thèse de Paris</i> , No. 89, 1858. Heyfelder, <i>Deutsche Klinik</i> , 1851, No. 35. Wolff, <i>Berl. Med. Zeitung</i> , 1858, No. 42. <i>Long. Med.-Chir. Rev.</i> , Jan. 1858. Marx, <i>Des Accidents Fébriles à forme intermittente et des phlébotomies sùge opérative qui suivent les opérations pratiquées sur le canal de l'urètre</i> , Paris, 1881. Rösser, <i>Archiv der Heilkunde</i> , 1867, No. 3; <i>B. and F. Med.-Chir. Rev.</i> , 1867, xl, p. 529. Fayer, <i>Clin. and Path. Observations in India</i> , 1873, p. 38; <i>M. J. and G.</i> , 1868, li, 175. Nunn, <i>Lancet</i> , 1871, li, 909. Banks, <i>Ed. M. J.</i> , June 1871. Holmes and Hulke, <i>System</i> , 3rd ed., lii, 211. Guyon, <i>Leçons cliniques sur les maladies des voies urinaires</i> , 1881, p. 420. | | | |
| | | | | Local or Sympathetic or Reflex Dystrophy. | Nephric changes invisible or inadequate. | Urinary Arthropathy. | Rheumatoid fever. Brodie. | Imperfect recovery. | Months. | Brodie, <i>Lectures on Diseases of the Urinary Organs</i> , 3rd ed., p. 27. Velpau, Civiale, Perdrigou, Marx, see above. Phillips, <i>M. J. and G.</i> , 1853, li, 1103. <i>Bull. de Thérapeutique</i> , tome iv, pp. 203-216. Rösser, Fayer, Banks, Guyon, Savory, see above. Berkeley Hill? Quain's <i>Dict.</i> , p. 1704. |
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| | Organic Lesion Nephritis. | Catarrhal. Rayer, Hamilton. Interstitial. Marcua Beck. | Moderate Pyrexia, Ischuria, Pyuria, Hæmaturia, Albuminuria, Emesis, Coma (Anasarca?). | Often fatal. | { One week to several weeks. | Rayer, Mercier, <i>Comptes Rendus</i> , liii, No. 21, p. 912. Hamilton. | | | | |
| | | | | | | | Functional Renal Lesion. Marx, Savory. | High irregular remittent fever, Hæmaturia, Albuminuria, Emesis. | Often fatal. | { One week to several weeks. |
| | Renal, indirect, by accumulation (Uremia). Sir H. Thompson, Malherbe, Guyon, etc. | (1.) Extension of Inflammation by continuity, or by chemical changes in urine. Rayer, Hamilton, Marcus Beck, Cameron. (2.) Change of Pressure in urinary reservoirs, leading to changes in renal and general circulation. Sir H. Thompson, Ogilvie Will, Cameron. (3.) Nervous reflex * action. Marcus Beck, Cameron. | Vaso-motor (Inhibition or Paralysis). Secretory (Inhibition or Paralysis). | Ischuria, Hæmaturia, Complete Suppression, Coma. | Occasionally fatal. | { Few hours to three or four days. | | | | |
| | | | | | | | Toxæmia. | Non-renal, direct, by addition (Ichoemia). Velpau, Sedillot, Harrison, Guyon, etc. | Septicæmic. Pyæmia. | Irregular remittent fever, rigors, nausea, high pyrexia, delirium. Irregular remittent fever, rigors, nausea, high pyrexia, delirium. (Metastatic abscesses.) |
| | Diseased. | Neurosis. Toxæmia. | As above | { The same forms as above, but more severe. | More fatal. | More rapid. | | | | |

Kidneys.

you would be unable to detect its presence. So the kidney may be inadequate or unable to produce a healthy urine, and yet, when examined by a microscope, or in any other manner which the advance of modern science has placed at our disposal, you may not be able to detect any structural lesion.

Now, after these preliminary observations, we are brought face to face again with this question of what is called Catheter Fever; and its exact place among the other effects of catheterism will be seen on reference to the table I have placed on the wall. I have two tables—one which is capable of being shown on the wall, and another which, being the crowded table of a busy man, is scarcely fit for representation in this way, but which may be useful in the course of the discussion, because it contains not only the facts shown on the table, but it contains the references to the various authors who have written on the subject, and to the pages in this book¹ in which the exact quotations will be found; and I will take the opportunity, Mr President, of placing this book and table in your hands, to use it as you may think proper. Now, a glance at the table hung upon the wall will show you better than any lengthened description which I can give you the exact position of what has been named for the moment “catheter fever.”

Let us now suppose that the operation of catheterism has been performed upon a person whose kidneys are assumed to be healthy. When injurious effects follow, they are first visible in the nervous system, and there they are suddenly or slowly developed. Of the effects which are sudden I may mention faintness, apyretic rigors, epileptiform seizures, suppression of urine, and coma. And of all such effects flowing directly from disturbance of the nervous system, it may be said that they bring about other effects, some of which exert a toxic effect upon the organism. Of the effects, which are slow, of catheterism upon the nervous system, the first and most striking is a sort of general dystrophy manifested by fever; the second is a disturbance of the nutrition and function of the kidneys which are associated by community of work with the urethra and bladder; and the third is the appearance of purulent arthropathies. When, as is often the case, the disturbance of the nervous system and the disordered metabolism consequent upon it are added to and aggravated by septic absorption, chemical or vital, then in lighter conditions we have septicæmia, and in graver ones ordinary purulent infection. Now, setting aside mere shock at the one extreme of this scale and pyæmia at the other, we have printed red in the table the conditions embraced by the term Catheter Fever. And of this it may be said that it pursues an irregularly remittent course; that it is not, as we now understand

¹ The book to which Sir Andrew Clark refers is in manuscript, and by his kindness we have been enabled to take from it the references to authorities found in the table.—ED. *Edin. Med. Jour.*

these fevers, either distinctly uræmic or pyæmic; that it lasts from two or three days to two or three weeks; that it is sometimes fatal; and that an autopsy reveals no definite visible structural lesion adequate to account for death.

The second and concluding assumption made in the table is that this catheter fever may arise in persons whose kidneys are diseased; and that death, when it occurs in such cases, is not necessarily or usually due to the previously existing renal disease, itself inadequate to account for a fatal issue, but to certain superadded and for the most part dynamic states brought about by the use of the catheter. The question, however, more immediately before us is the occurrence after catheterism of a fever in which the kidneys are alleged to be free from disease before the operation. Does such a fever really exist? Is it true, or (not to use a term involving moral considerations), is it accurate to say that in persons of ordinary health the use of the catheter is sometimes followed by fever, and that when this fever has a fatal issue the autopsy reveals no visible structural lesion adequate to account for death?

I conclude, for my own part, that such a fever does exist, and the grounds for this conclusion are these:—*First* of all, the admissions, setting aside diversities of interpretation, of almost all—not all, but almost all—authors, from Moffatt in 1810 to Guyon in 1883. And it has been particularly, and I would say admirably, set forth by a person who has been strangely overlooked in this discussion, namely, Mr Banks of Liverpool, whose communication appeared, curiously enough, in the *Edinburgh Medical Journal* of 1871. He is, as I understand the matter, the only English author who has clearly recognised the existence of this fever, and he has alleged that it is quite a distinct fever from the fever of what is called pyæmia. *Secondly*, I believe in its existence from the observation and post-mortem examination of three cases of my own, and of many other cases in which, unfortunately, no post-mortem could be obtained. *Thirdly*, I believe in it on account of the complete recovery of some cases of serious fever following catheterism, recovery being barely reconcilable with our knowledge of the clinical history of severe *interstitial* nephritis. Furthermore, I assume its existence from the fact that in many post-mortem records of old-standing hospital cases dying of prostatic vesical disease—for example, in 26 out of 66 recorded by Marcus Beck in the University College Hospital Records—no evidence of renal disease has been recorded. And finally, from the fact that in the cases where renal changes exist they are not, as proved by the examination of the urine and by a study of the general symptoms, adequate of themselves, or with the help of any other visible structural lesion, to account for death. I say then, to use the mildest term that I can on such an occasion, it is at least extremely probable that this catheter fever exists. And if it does exist, how are we to account for the allegations of certain

specialists of great and undoubtedly just distinction, that in fatal cases of this fever renal disease has always existed before the use of the catheter, and is always, in fatal cases following its use, the cause of death? With not a few persons, sir, such a statement from such quarters is conclusive as to their truth. But truth, which has no creeds, certainly bends to no authorities, and in such contentions as this in which we are engaged, the appeal must be made, and always made to facts, and not to mere opinions—to reasonable probabilities, and not to particular persons. Besides, I hope it is not unreasonable to say that one may be too near a subject to see it quite accurately. Sometimes, indeed, it requires one to go a little distance in order to see a thing in its just proportions to the other things of which it forms a part. Nay, even that is not always alone sufficient. Sometimes it needs an intelligent and interested bystander to set one right as to the conceptions which one forms of the things in the midst of which we are engaged. I suppose that in a battle those who are in the midst of it scarcely know the disposition of the forces or the manner in which the tide of victory is turning. It is the general and the staff—standing at a distance, having all the field in sight—who alone can determine this. Now, that in fatal cases of catheter fever the kidneys are not always diseased beforehand I conclude from the following facts:—*First*, From reported cases, to which at least a certain amount of weight must be given. *Second*, From examinations of urine made by competent persons before the entrance on catheter life. I may here pause for a moment to relate a case which, by the kindness of Mr Howard Barrett of London, I am enabled to do. The substance of it is this:—A gentleman came to Mr Barrett and consulted him for a little incontinence of urine. He examined the urine, and, being a man of a scientific turn of mind, and one of the most careful men I know, he examined it with a care and precision which are to me absolutely trustworthy, and he declared that this specimen of urine was healthy. Seeing this, he recommended some simple means to allay the irritation of the bladder, and dissuaded the patient from surgical interference. But Mr Barrett's advice was not followed. The gentleman consulted a surgeon, who followed the usual course of treatment in such cases by placing him upon catheter life. In a short time he suffered from catheter fever and died, and the report of the cause of his death was advanced disease of the kidneys. *Thirdly*, From examinations of the urine made by myself. I am accustomed to examine the urine, and it has occurred to me, as it has to every busy physician, to have been consulted along with surgeons for maladies of this sort. I have on all such occasions—either myself or by the help of my assistant, Dr Sheridan Delépine, whose technical skill, acuteness of observation, and exactitude in description, are well known in Edinburgh—examined the urine, with an eye to the existence of

renal defects. In many such cases we have found what I call conclusive evidence of healthy kidney—that is to say, a normal amount of urine of normal constitution—and no evidence of any constitutional state or local disease which might influence unfavourably the renal secretion. *Fourthly*, From the complete restoration to health, which in some cases follows recovery from catheter fever, a restoration which can scarcely be expected in true and considerable interstitial nephritis,—a restoration, however, which I admit might be possible in the affection which is now called interstitial nephritis, and which is a very different thing indeed from the so-called interstitial nephritis of ten years ago. It seems to me that the interstitial nephritis of these days consists in little else than in changes, for the most part degenerative, undergone by escaped leucocytes lying between the tubules and around the vessels. However, that is meantime beside our question. *Fifthly*, From the fact that in some cases of fatal catheter fever examined before catheterism, no evidence of renal disease, although especially sought for, could be found in the constitutional state of the patient any more than in the composition and characters of the urine.

Now, as regards the second contention, that death is due always to renal disease, and that even if the catheter fever were recovered from, the patient would die soon after in the usual way from uræmia, my experience compels me to dissent from both points. Probably most of my hearers will know that, in a certain work of the highest merit on vesical diseases, there is a paragraph which says, that in cases where death follows the use of the catheter it is certain that renal disease has existed before the use of the catheter, and that if the patient had escaped the fever which ends in death, the renal disease would have before very long terminated his life. It is true, no doubt, and no one has doubted, that in long-standing cases with dilated ureters and contracted kidneys death does sometimes arise from uræmia. That is true, but to state the same thing of recent cases is not true; for in recent cases neither the general symptoms nor the characters of the urine in the closing days of life resemble in any manner those of true uræmic poisoning. Hear, for example, the words of Marcus Beck, who is an advocate of what is called the nephritic view of all these fevers, but who, with the candour which becomes an honest and great inquirer, makes this admission. Mr Beck, speaking of the closing days of urinary fever, or, we may say for the moment, catheter fever, says:—"At the same time"—that is, at the same time that he asserts the existence of renal disease—"the secretion of the urine continues in fair quantity, and the patient cannot therefore be said properly to die of uræmia." And further on he adds, "As before stated, in many cases the real cause of death is not the damage to the kidneys, for the excretion of urine remains abundant to the last, but the absorption from the abscesses or from the putrid urine

in the pelvis. On the other hand, the symptoms closely resemble, and, as a matter of fact, are often undistinguishable from those of septicæmia." Furthermore, it is not, I think, accurate to say that, if these patients escaped the fever they would necessarily in a short time die. I venture to say of renal disease, as I constantly say of heart disease, that the prognosis has to be re-written. The most striking illustration of this assertion that I can give you, sir, is one that occurred in the person of the wife of a distinguished Arctic voyager. Somewhere about the year 1840, Robert Christison, the great physician of this city, discovered that she had renal disease, and there was no doubt of the fact—if any doubt could attach to a diagnosis of Christison's—for it was confirmed by physicians in London and Dublin. And yet she continued to live, to work, and to work actively, and to enjoy life for over thirty years after that time. As a matter of fact, the prognosis of renal disease, as of many other organic diseases, is recorded in books by men who are beginning to get their experience, and not by men who have got their experience. The prognosis of renal disease and of heart disease is usually formed from the study of those diseases in the wards of an hospital. But any one who knows well the sick poor knows that it is only at their last extremity that they go to the hospital, and that when they enter the wards with cardiac or renal disease, and when they say they have been for a certain time ill, that is no true record of the time during which symptoms of their disease have existed, and which would have been discovered sooner if the opportunity had been offered. They go into the hospital when they cannot subsist out of the hospital any longer. And so it has come to pass that mitral disease has been stated to last from four to five years, when experienced physicians know that, without alteration of health, it may last forty. So likewise it is with renal disease. I have myself heard a distinguished teacher say that two and a half years was the duration of life of a person with chronic disease of the kidney. And yet I, who enjoy some of the fair privileges of age, can look back to the case of an alderman of London, who, after I had appointed him to die in 1868, is now living and in the enjoyment of tolerable health. Then if it be true that there is such a fever as catheter fever, and if it be true that there is in certain of these cases no visible adequate structural cause to be found for death upon the autopsy, and if, furthermore, it is true that in certain cases where the kidneys are affected the affection is not adequate to account for death, the question arises, What then is the differentia in these cases? What is that state which, without renal disease in some cases, and which superadded to it in others, occasionally after catheterism brings about death? For the moment I shall, as I have already stated, call it catheter fever. Obviously it is a clinical, and, as obviously, it is not a patho-

logical entity. What is its nature? I do not know. I can state only facts, and I can interpret them only by reasonable conjecture. It would require, sir, a greater amount of youth and a greater amount of inexperience than I could justly lay claim to, to speak with confidence upon this question. I have no doubt that some who can lay claim to both these qualities would do so. I dare not. But so far as I may judge, I should say that there appears to be three steps or stages in the evolution of this so-called catheter fever, occasionally perhaps four. *First* of all it seems to me that there is some direct or reflex irritation of the nervous system. *Secondly*, That through that irritation, or it may possibly be independent of it, there occurs a disturbance and disorder of all the metabolic processes throughout the body, and the various organs and tissues, but, of course, more particularly in the kidney, on account of its communal relations with the urethra and bladder, which are themselves in this case the object of irritation. *Thirdly*, A septic infection, occurring or begotten, either auto-genetically or deuterogenetically begotten, inside or outside the body. *Fourthly*, And occasionally—for it is only occasionally that it occurs—I think that the sudden diminution of pressure, by the removal of a large quantity of urine from the bladder, brings about such a change in the constitution of the vascular supply and the nervous state of the bladder, ureters, and kidneys, as sets in motion all those disordered metabolic processes which lie between the nervous disturbance on one side and visible structural lesion on the other.

Now, sir, it seems to me that the difficulties of this subject—and they are greater than may even appear from this statement of them—have been unnecessarily heightened by the disproportionate attention which has been given to anatomical changes. Such disproportionate attention is not peculiar to this subject. It is perhaps one of the marks of the medical life of the age in which we live. And this disproportion has been, as I think, brought about and maintained by the objective character of the inquiry, which is always pleasanter to men than abstract subjects of inquiry, by the increasing facilities which have been given for such investigations, by the ease with which they may be pursued, by the comparatively moderate ability and knowledge necessary to success, and by the abiding reputation of success when success has been achieved. It is not a very uncommon thing to see a person who, in the early part of his life, has written a successful paper on an anatomical, or, properly speaking, pathological subject, and has rested upon his laurels, and thought himself entitled for ever after to sneer at clinical medicine as an empirical and unworthy thing. It would almost seem sometimes that the great end of rational medicine was the mere description and explanation of structural changes. But surely, sir, this is a fatally inadequate view of scientific medicine. For structural changes, at least as far as I can read them, are all episodes,

yes, and often but uninteresting and unimportant episodes, in the evolution of disease. Disease, as I see it at least, in its beginning, in its continuance, in its issues, in its endings, is dynamic. Its physical, chemical, and for a moment, if I may be pardoned, vital changes, and the interaction of such changes upon each other beyond the scrutiny of mere physical things, precede, originate, maintain, underlie, and give form, meaning, and importance to all pathological changes. The question is not what the structure teaches, but what the conditions begetting that structure mean, for it is they that constitute disease, of which structural changes are but inadequate signs. And here, if I mistake not very greatly, in this domain of dynamics lies the great future of physiological, pathological, and clinical discovery. But it is certain that such processes must be slow. They demand the highest faculties of the mind for the purposes of such inquiry. They require qualities rarely found together, accurate observation, a deep and wide insight, ingenuity of interrogation, a ripe sobriety of judgment, infinite patience, and complete unselfishness. The toil, obviously, of such inquiry is exceeding great, the fruit produced by each individual very small, and the return, beyond the use to others and the pleasure of the toil, utterly insignificant.

But to return from this pleasant and seductive digression. Let us assume that catheterism is sometimes a perilous operation, and that it may be followed by fever, ending sometimes in death, and furthermore assuming that one is about to deal with a middle-aged man suffering from an enlarged prostate and irritable bladder, the following problems at once present themselves for solution, and it is these problems that I now respectfully submit to your consideration. First, then, what circumstances should determine the habitual use of the catheter, and what circumstances should determine one to avoid its use? I must say that I have received communications of cases in which it appears that it is not always necessary in certain conditions, in which it is usually thought necessary, to put the patient on catheter life. Mr John Merriman, one of the most distinguished general practitioners in London, communicated to me a case of which I will give the substance. He told me that he had had a patient suffering from irritability of the bladder, and a little incontinence, and that he had endeavoured to introduce a catheter, and that finding that it was followed by considerable local irritation in the shape of catarrh, and generally in the shape of a few rigors, he abstained. But the irritability continuing, and the friends becoming impatient, they called in a surgeon to consult with Mr Merriman. The surgeon proceeded with some little force to introduce a catheter. Twenty-four hours afterwards the patient had a severe rigor, and for fifteen or sixteen days he lay in a serious fever, as Mr Merriman thought, in near peril of death. At the end of that time he began to recover, and, curiously enough, not only recovered from the fever, but from his

local trouble, and from that day, as far as I know, until this day both the catarrh and the irritability of the bladder have entirely ceased. So that it happens sometimes—I am afraid it does not happen very often—that in cases of irritability of the bladder and even fever, the whole of the symptoms may subside without the intervention of catheter life. Secondly, if it is resolved to place the patient on catheter life, what local and what general precautions can be taken, with any reasonable hope of averting this catheter fever; and, thirdly, and if, in spite of all these precautions, this so-called catheter fever should arise, what are the means most likely to guide the case to a successful issue? And now I will no longer trespass upon the time and patience of the Society. I commit the subject to your consideration, assured that it will receive from you a just and an adequate discussion, and believing that the issues of it will tend to the improvement of our knowledge, the better relief of suffering, and the greater safety of life.

Dr Patrick Heron Watson—I feel that I have been called upon to occupy an important position at this period of the evening which I think many surrounding the table might, more worthily than I, have been asked to accept. But, without further expression of vain regrets, I will at once proceed to say that I express, I am sure, only the feeling of the meeting when I state my sense of the obligation under which we are laid to the eminent gentleman who has read the eloquent paper of to-night. And in the eloquent—I may almost say poetic—remarks with which he prefaced the paper, I am sure that Sir Andrew Clark has rendered to the Edinburgh School and to the eminent teachers with whom he has been associated in the past, a just tribute of admiration for which the meeting thanks him, and with which we all entirely sympathize. The subject which Sir Andrew Clark has so ably and so eloquently introduced to our notice this evening is one which, I believe, has exercised the minds of surgeons, pathologists, and clinical teachers here as elsewhere. I am confident that the remarks which Sir Andrew Clark made on another occasion and in another place, with which we are all acquainted, and which were received by the public with the deepest interest and with the highest respect, have probably placed the matter in a more definite and crystallized form than the wandering ideas that we had permitted to enter into our minds could be expected to afford. The matter is one of great importance both as regards the health of the patient and his prospects of vitality; I think, then, we cannot express our thanks too deeply to Sir Andrew Clark for enforcing the matter on our attention. What is this condition which is the subject of discussion to-night, and which Sir Andrew Clark had designated “Catheter Fever?” It seems to me that as it has been described there is one thing to be specially noticed, and that is the introduction of an instrument along the urethra—an instrument possibly

introduced once, or it may have been much oftener,—the patient having even entered for some time on what is called “catheter life,”—before any unfortunate symptoms have shown themselves. When this condition of constitutional disturbance occurs its initial symptom is usually a rigor. This is not immediately after the introduction of the instrument, but usually when the patient is first making water after the instrument has been passed. We often find, indeed, that it is not at the first time of passing water. He may make water frequently before that, but it is in the course of half a day or an entire day after the use of the instrument for the first time that these symptoms of rigors usually manifest themselves, and the rigors are followed by more or less febrile symptoms followed by copious diaphoresis, and more or less complete return to the condition in which he was before the rigors occurred. These phenomena may repeat themselves on each endeavour to micturate. Now, undoubtedly, this may be said to be the outset of what we may call the physiological manifestation of the condition called catheter fever. There is one other initial fact I wish at once to advert to before going further. So far as my own experience goes, though undoubtedly it has been limited very much to one sex, catheter fever is only met with in the male, and not in the female. In the female sex one does not meet with the rigors and fatal issues after the passage of a catheter. This has been my experience. Now, to return, in the cases in which we have these conditions existing, there are first a rigor and next the fever. The character of the fever that manifests itself may be either slight and ephemeral, or in certain circumstances it is of a much graver character. Where we have this form of fever followed by such disastrous effects as those to which Sir Andrew Clark has referred, you will find that the temperature is sometimes as high as 105° F. The fever from which the patient suffers is sometimes of an absolutely typhoid character, as that term was used by the older writers, in which the patient lies with a brown shrunken tongue, an incoherence in speech and muttering delirium. Now let us look at a few of the effects of urethral irritation as bearing on this ailment to emphasize that warning. How frequently has it been impressed on all young practitioners that a man who has the catheter passed for the first time should be in a recumbent posture! It has been said that otherwise there is a risk of his tumbling down on them; as the instrument passes through the posterior part of the canal it gives rise to a storm of the nervous system, which, inhibiting the heart's action, causes syncope, and this is in some instances followed by epileptiform seizures, with convulsive movements and twitchings of the face, the patient usually recovering when he has reached the horizontal level. In such circumstances it by no means follows that this storm is followed by any further development of febrile disturbance. But it points to a urethral factor in the disturbance of the nervous system which Sir Andrew Clark pointed to as the origin of the

whole of the febrile symptoms which followed in the wake of passing a catheter. Let me, in connexion with the aetiology of such a class of cases, advert to an old established method of treatment in cases of suppression of urine, viz., to pass the catheter to set up a reflex stimulation of the kidneys. But in other instances we find that after the passage of an instrument there is no recognised disturbance of any kind. Then the rigors were probably not of a very marked kind, but it is observed, however, that there is a diminution in the quantity of urine. As in a case to which I shall afterwards refer, the patient may have passed as much as 120 ounces before he enters on his very brief catheter life. After the use of the catheter the 120 ounces were diminished day by day by 12 ounces, and at the period of his death, eight days afterwards, it was reduced to a comparatively small amount. The urine had ceased to present the aspect of urine; it presented a muddy, brownish red appearance, largely charged with blood corpuscles and pus, and a proportionate amount of albumen. But before the passage of the fatal instrument the man's urine, beyond its being large in quantity and of low specific gravity, showed nothing which would enable one to judge that the patient had some condition in connexion with the kidneys which might cause serious consequences when he entered on catheter life. Then another fact is that the patient may die very rapidly after the development of the first rigor. I have known a patient die within three days without a very marked diminution of the urine, and without the presence of any abnormal constituent except blood. This blood, which from its characters may have been presumed to be owing to some slight ruffling of the urethra in the introduction of the catheter, comes from the renal tubules, possibly from the Malpighian corpuscles. But we may find that death is much more speedily induced after the passage of a catheter. No long time ago the death of a well-known nobleman convulsed the upper ten thousand. He was a man perhaps a little nervous about himself, and he was at that period of life when it might have been supposed that he had some obstructive affection of the urinary organs. He was recommended to consult a surgeon, who passed a bougie. Thereafter he was making water all day, and he sat in front of the fire and complained of feeling ill. He was seen by a distinguished medical man, who repudiated the idea that there was anything very seriously the matter with him. He was not inclined to go to dinner, but was persuaded by his daughter to take some dinner, and with it a glass of champagne. He retired for the evening, and was found dead in the morning, his last act in life having apparently been to micturate. After death an examination of his body took place, and there was found disease of the heart, and a calcareous aorta. There is one other thing I have observed again and again in this catheter fever: when the symptoms have been far advanced and the patient sunk low, a herpetic eruption has come out on the face coincidentally with recovery.

How are these phenomena to be explained? Is the passage of the catheter essential to the development of any of these symptoms? I am inclined to answer this in the negative. You are perfectly aware that other conditions may give rise to it. The passage of a calculus along the urethra may cause rigors and disturbance of the system. The passing in large quantities of uric acid or oxalate of lime, the passage of urine with blood clots, or the passage of air and gas, without there being any decomposition of urine, may give rise to the development of similar results. It is a familiar circumstance that where a patient has suffered from stricture of the urethra he is liable to febrile attacks, which are spoken of as exactly analogous to ague in their nature. Such was the character of the ailment of the great Henri IV. He had attacks of rigor and fever. He was treated for ague, but he suffered from a stricture of the urethra, and the aguish attacks were symptomatic of nothing but that affection. Then there is another symptom which is not far dissevered from this form of fever, and that is the affection of the joints which sometimes follows a gonorrhœa, and known as gonorrhœal rheumatism. The time it comes on is between five and eight weeks after the commencement of the urethral disease. The knee is the joint most frequently affected, but we may have the sheaths of the tendons and other parts, and even the membrane of Descemet affected. The whole anterior chamber of the eye may be filled with flocculent matter. But the great peculiarity of it is that, though it is apparently closely allied to pyæmia, and though attended with a flocculent effusion, it is rarely or never followed by suppuration, and the joints are usually restored to their normal condition under suitable treatment. This catheter fever is not due to any severe injury that has been inflicted on the canal. We don't meet with it where the urethra is ruptured, or where false passages leading to urinary extravasation have occurred. Rupture of the bladder itself is not by any means accompanied by symptoms such as we have heard described so graphically to-night. It is not due then to absorption of urine or to any purulent condition. In other words, it is not due to uræmia nor in any way to a *quasi* pyæmia. Is it due to the introduction of septic germs?—an idea not unnatural in those days when every disease is put down to the influence of such elements. In some cases where an antiseptic catheter alone has been employed, we do not find that these fatal results are altogether avoided; nay, I will say this, that where an antiseptic catheter has been alone employed and the patient has died, we have found numerous minute abscesses scattered throughout the cortical substance of the kidneys containing bacteria and micrococci, and I have had an opportunity of exhibiting to the Society a specimen of this condition of matters in which a surgical kidney had it so well developed that you might almost have called it a diphtheritic condition of the kidneys, ureters, and bladder. Then the condition is certainly not connected with ague,

for it occurs in persons who have never suffered from such conditions. I am myself inclined to attribute it to irritation of the prostate, which causes reflex effects on a spinal centre telling with most injurious influence upon the Malpighian glomeruli, the capsules, and also the system of capillaries which pass along the tubules towards the radicles of the renal veins. In these cases, where death occurs, we may find minute clots in the vessels, and leucocytes and red corpuscles occupying the spaces between the tubules, blood extravasated into the tubules themselves with an entire absence of all epithelium from the tubules, blood in the urine, and a marked diminution in the amount of urine. As to the conditions which precede and give rise to this state of things, surgeons believe that it is met with most frequently in connexion with an enlarged prostate. I will now show a melancholy specimen of the last case of a patient's introduction to catheter life that came under my care. He was from the country, 53 years of age. He looked pallid, transparent, bloodless, and complained of constant thirst. He had been supposed to be suffering from diabetes, but on my examination of the patient I found that his bladder was distended to the umbilicus, and I came to the conclusion that the exhausted state of health was to be attributed to conditions evolved out of that. It was an antiseptic catheter that was used. For two days after introduction to catheter life he remained perfectly well. The urine diminished from day to day by 12 ounces. The last day almost to *nil*, only a little grumous bloody fluid coming away, and the patient died. The bladder is a bladder of an old-standing obstructive condition in which there is no prostatic disease. You will notice that the ureters are very much distended, that the kidneys are very large, and that there are scattered throughout the kidney innumerable small abscesses as a result of the interstitial nephritis. The specimen I think is apposite to the occasion, and may be an object of interest. Now, after all I have said, I come to the conclusion that I would rather not call this fever Catheter Fever, but Urethral Fever. It is to my mind a fever which is dependent on, it may be a catheter, or something else, a calculus, bloodclot, sand in urine, which irritates the prostatic part of the canal. In some instances this catheter fever is both trivial and evanescent in its effects, and in such cases I can agree with the deadly error into which surgeons have fallen, and say that in those cases in which the affection is trivial and evanescent the patient is suffering from no important pathological lesion; that where high fever and serious structural results occur, the dangers are chiefly to be found in a feeble heart, diseased arteries, kidney disease either original or as the result of distension from below; that in such circumstances, where this condition of fever is set up as the result of the prostatic irritation, it is the pre-existence of serious structural disease which is the turning-point in the history of the case as to whether the patient shall recover or not. The

prognosis of these cases may be determined by the rise and persistence of temperature, and on a careful investigation of the patient's urine from day to day. In treatment, as we certainly find that the sudden emptying of a largely distended bladder, with probably distended ureters, is exceedingly apt to be followed by serious consequences, and the use of a rigid metallic instrument is more likely to give rise to febrile circumstances than a soft instrument, and septic consequences complicate the result, a soft catheter and an antiseptic lubricant should be used. And as regards remedial proceedings, my own experience is in favour of the employment of quinine and digitalis for some days before the first introduction of the catheter, and bromide of potash or an opiate for twenty-four hours after its employment. The use of the perchloride of iron throughout the whole period of the fever is what has afforded me the most satisfactory results, while the use of vapour baths, or hot lamp baths where the patient's skin could be got to act, I have found invaluable. I again desire to express the sense of the obligation under which the Society is laid by the very able and interesting address which Sir Andrew Clark has given us.

Professor Grainger Stewart. — After the very able speech which Dr Watson has just delivered, I shall not detain the Society for any length of time discussing the different aspects of the subject proper brought before us, or of those very interesting side issues which occurred so appropriately during the delivery of the address. There were many things on which one would gladly have the opportunity of having a prolonged discussion besides the subject of catheter fever proper, but what I have to say will be restricted to that. Of course it is but slowly that a physician gathers experience of these things. Surgeons have seen more of these cases when they have been in practice for a year or two than physicians have after twenty-five years' experience. I have certainly seen three different conditions: — *First*, That which has been described so well, the acute rigor or other formidable symptom following the introduction of an instrument into the bladder. Such an attack cannot be attributed to anything except reflex influence through the nervous system. *Second*, There is a condition which is met with as a sequel of this operation, and sometimes results from long-standing disease in the urinary track. It consists in extensive sloughing, or a low type of inflammation of the mucous membrane of the bladder, the organ containing quantities of mucus and blood, the ureters and the pelves of the kidney acutely inflamed, and the substance of the kidney itself studded with abscesses, in fact, the surgical kidney. And *third*, between these two there is another form of disease which I have had an opportunity of studying, and to which I believe Sir Andrew Clark wished mainly to direct our attention. I think I may best speak of it by recalling a special case. Not long ago a gentleman came from the country to consult me

about excessive micturition and incontinence of urine. He passed about 150 ounces every day. I found that the patient, besides his renal troubles, had disease of the aortic valves, also a good deal of emphysema of the lung, and acute catarrh of the stomach. Under treatment he improved considerably in respect of his stomach, and in respect of his heart and his breathing, I think, became easier; but the urinary symptoms continued, and I thought it right to ask a friend to draw off the urine. This was done, and the patient experienced considerable relief. The quantity of urine continued distinctly for a week to go down, and that in a favourable way. Coincidentally with the diminution of the quantity other symptoms improved. I wish to refer to this kind of polyuria. What is it that makes these patients with old-standing prostatic disease pass so much water? So far as I have been able to see, it is associated with the dilatation of the ureters and the pelves of the kidney. I have seen polyuria in a simple case of dilatation of the ureters. In that case it appeared that the hydronephrosis gave rise to polyuria. Perhaps the explanation is, that in these conditions the counter pressure upon the Malpighian tufts is diminished, a greater transudation is the result. Well, in the case of the gentleman to whom I have referred, a very favourable change went on, and he was able to return home. He had been taught to use the catheter with antiseptic precautions, for I believe in the antiseptic treatment. He came back to me about a week later. His urine had become somewhat milky, and it had a little trace of pus in it. He came again when he was manifestly unfit to come from the country at all. He went home, and died in the course of a day or two. A post-mortem examination was made by two gentlemen at my request. One of them was well accustomed to such examinations. They found that the prostate was greatly enlarged, the bladder dilated and inflamed to a considerable extent, the ureters dilated, their walls congested, and the kidney substance apparently quite natural. Now, the question comes to be, What is to be the explanation of the fever and the fatal result in such cases as this? *First*, I have no doubt that a certain influence is to be assigned to reflex action through the nervous system. Then, *secondly*, I think that some importance must be attached to general disorder of the system following on the nervous irritation. *Thirdly*, I think there is considerable importance to be attached to the septic condition. I believe that I have never seen anything that could be properly called uræmic, but there are, on the other hand, distinct evidences of what we would call septicæmia. As to the *fourth* element,—the altered state of the vessels and mucous membrane in the kidneys, ureters, and bladder,—I hold that it is also of considerable moment. Where there had been a habitual over-distension of

these parts, and when it is removed and the parts fall together, there must be an extraordinary congestion of the mucous membrane. Then the surfaces of the membrane which had been kept apart come into contact with one another. Various bacterial or other elements have the opportunity of developing in the folds. This is, I believe, an important element in the disturbance, and a constitutional septic process and an irritation of the periphery of nerves may be set up. Now, with regard to management, I wish to say how heartily I endorse the recommendation of rest, and quiet, and low diet, in those who are beginning their catheter life. I also believe in the use of such remedies as Dr Watson has indicated, but unless there are good grounds for avoiding them, opiates are eminently useful.

Professor Chiene.—I need not say that, along with many others in Edinburgh, I read with very great interest Sir Andrew Clark's London address, as well as the discussion which followed that eloquent address. I listened to-night to the eloquent remarks Sir Andrew had made in connexion with the same subject, and the result of a consideration of the cases he has brought before us leads me to suppose that the patients in these cases died of septicaemia. Sir Andrew has detailed in the London address three cases—one occurring in the year 1850, one in 1865, and the third, that of a nobleman, at a later period. Two of those cases were fatal, but the nobleman recovered. He has to-night given two other cases,—one which occurred in the experience of Mr Howard Barrett, the other in that of Mr Merriman. These I must take as types of the disease Sir Andrew has so well described to us. I wish at once to say I accept his facts, and I do not wish to discuss the question in connexion with unhealthy kidneys. I accept the position in connexion with a perfectly healthy kidney, and the catheter as passed in consequence of some trouble in the passing of water. Now I have to go back in this matter to about 1865, and I have brought two preparations of the bladders of patients who died in my dispensary practice. These two patients died between 1865 and 1868; and, in the year 1869, showing the preparations to Dr Wyllie, I asked him if he could explain how these patients died? I may state that they died with symptoms such as Sir Andrew Clark has described. Dr Wyllie said he believed that I had introduced septic matter with the catheter. Although I acknowledged his explanation, I at first did not believe him. The urine looked healthy the first time I drew it off, it was healthy the second and third times, and then it became muddy and ammoniacal. The remark Dr Wyllie made was this:—"You introduced septic matter into the bladder, and the patients died." I have since observed many cases, and I have to say that I feel more and more sure that the statement of Dr Wyllie was correct. I also believe what Sir Andrew Clark has said that a person might die from irritation of the urethra, and so on. But I believe that

the great majority of the cases are septicæmic; and I am very pleased to find in Sir Andrew Clark's London address the—to me comforting—statement that the urine of the catheter fever is always loaded with micro-organisms of various kinds. I am pleased to hear that fact, because it supports the theory brought under my notice by Dr Wyllie, and of which I have seen abundant evidence by clinical observation. The condition then is a septic one, and the septic matter may enter the bladder in four different ways. *First*, It may be introduced by an impure instrument. *Second*, It may be introduced also by air rushing in along the catheter. It may enter, *thirdly*, by passing along an inflamed urethra, which has been irritated by a prolonged use of instruments. We have at present a case in the wards, a man suffering from fracture of the spine, whose water has had to be drawn off. His urethra has become inflamed. A "carbolic gonorrhœa" has been set up by the irritation of the carbolized instrument. Organisms have passed along the urethra, and a putrefactive cystitis has followed. And, *lastly*, organisms may pass between the catheter and the wall of the urethra where the catheter is tied in. These are the four ways in which the septic matter may pass in; and I will just say that the methods that are now adopted to purify instruments in this city are such as to tend to the prevention of catheter fever. Sir Henry Thompson used hot water for that purpose. If you boil the catheter, and put it in cotton wool till it cools, and then anoint with weak carbolic oil afterwards, it is safe. But if you simply put the catheter into boiling water and lay it down to cool, as it cools a time must elapse before it can be introduced into the urethra, and septic organisms may have fallen upon it, and may thus be introduced along with the instrument. I believe it is better to purify the catheter with 1 to 20 carbolic lotion. This year, in consequence of observing Koch's experiments, I have used for the purpose corrosive sublimate. I have not yet found an antiseptic oil which will but slightly irritate the urethra. Lately I have tried eucalyptus oil in the proportion of 1 to 6. There is one remark I wish to make, and I trust that Sir Andrew Clark will excuse me. He has said that these points have not received adequate expression in surgical works. I have only to say that Sir Andrew will find the points I have stated in the small work which all the students in my class get as a text-book for the first part of the course; and I have alluded to catheter fever as a form of septicæmia since I became a lecturer. I endorse every word that Dr Watson has said. We are greatly indebted to Sir Andrew Clark for his address. To show the importance attached to the subject, I show a set of instruments that have been devised by one of my students, Dr A. O. Ward, in consequence of what I have taught on the subject. They are catheters, constructed so that no septic matter can accumulate in the hollow below the eye of the instrument.

Professor Annandale.—I wish to be allowed, first of all, to add my most cordial thanks to Sir Andrew Clark for delivering his address to us to-night. My remarks will be very brief and practical. In the first place, I shall confine the discussion to the object Sir Andrew Clark had in view, that is, to bring before us the special form of urethral fever, in which no apparent disease of the kidney can be discovered.

Sir Andrew Clark.—No apparent disease adequate to account for death.

Professor Annandale.—I am quite prepared to admit that this condition can occur, and I will direct my attention to the four causes which produce that condition, and, if you will allow me, as a surgeon to give suggestions how to prevent that condition occurring. Well, in the first place, Sir Andrew Clark has mentioned the local irritation by the passing of the instrument. I quite believe this may be the case, acting as a reflex irritation, whether the prostate is enlarged or not, and no matter what the condition of the urethra. In the second place, the second cause is the result of that local irritation on the general system. I believe in this also. The third cause is the septic results which may follow the introduction of catheter, which I also believe in; and, lastly, there is the sudden emptying of the bladder and ureters by drawing off the water. I shall now simply venture to offer the Society a few hints how to prevent these causes. The first is, never to pass an instrument into the urethra or into the bladder without some care, and without knowing something of the patient beforehand. I have learned from experience the danger of not attending to this; and I will say that when a patient comes to you suffering from stricture, symptoms of stone, or other condition, you should never pass instruments till you have inquired into his condition or history. Another point is, that in passing an instrument you should take care that it is properly warmed. I believe that mischief is often done by the introduction of a cold instrument in producing a shock of the nervous system, and which may possibly bring about some of these changes. In regard to the third cause, I am quite at one with Sir Andrew Clark, and that is that great care should be exercised in passing instruments, and that antiseptic means should be used to prevent septic matter entering into the bladder. Then as to emptying the bladder too quickly, this may sometimes be a cause of danger. Therefore I would advise that if you have a patient whom you do not know very much about, instead of passing the catheter to relieve the retention, you should rather aspirate the bladder above the pubis. I think that relieving the retention by aspiration is, in some cases, safer than by passing a catheter into the bladder. I would again impress on every one the importance of this—not to pass a catheter or instrument into the bladder without knowing something of the patient. I have known cases where patients, dis-

satisfied with their ordinary physician or surgeon, have gone somewhere else. They have entered the consulting room of another surgeon, had an instrument passed, and the results have been very serious.

Dr Wyllie.—Mr President, you will understand from what Professor Chiene has said, that I agree with the view he had stated with regard to the nature of most of these catheter fevers. The subject, as the Professor indicated, attracted my attention a good many years ago when I was house physician at Birmingham Infirmary from 1866 to 1868. There was then a large number of medical cases, and among them not a few suffering from debility, old age, and enlargement of prostate. I remember speaking to one of the hospital surgeons on the subject, and he added another case to the two or three that I myself had had recently under observation. He said that just a few weeks or months before he had been called to the north of England to see a friend who had been under treatment. He stated that he had passed a catheter, and that his friend was now dead. Now, in common with Professor Chiene, I have paid much attention to the subject since 1868, but I will not attempt to detail cases or to take up the time of the meeting in any way. I would simply say a few words as to the possibilities of the introduction of germs into the bladder. No doubt the common method by which germs might be introduced is in the oil which anoints the catheter, but there was also the possibility of air getting into the bladder. I have known cases in which, in an attempt to assist the bladder in expelling its contents by pressing above the pubis, the hand has been lifted up before the catheter was withdrawn, and immediately the negative pressure of the abdominal wall sucked in a quantity of air. The greatest possible care is necessary to prevent the introduction of germs. I may refer to a case that occurred in the practice of Dr Archibald Dickson, in which an old gentleman had been overcome by the shock of the Glasgow bank failure, and presently required the use of the catheter. Dr Dickson was aware of the risk of introducing germs into the bladder; but when he was introducing the catheter and pressing the abdominal wall, the patient suddenly coughed. The rectus abdominis contracted, and in so doing raised the hand. Air was sucked into the bladder with an audible sound. The result was that in this case putrefaction set in, and the patient died. These are illustrations I would like to make of the fact that it is possible to introduce germs into the bladder, even although the greatest care was taken.

Mr Joseph Bell.—I would be exceedingly loath to take up the time of the Society and keep the members from hearing Sir Andrew Clark again, but there is one point to which I would like to advert, which has not been taken up by any of the speakers, and on which I felt very strongly, in connection with Sir Andrew Clark's observations. I believe that every one in the Edinburgh

School had, since the very valuable paper by Dr Wyllie, and also by Professor Chiene, of which he has with characteristic modesty said very little, I think about the year 1869—since that date I think that nearly every younger member of the Edinburgh School had tried to keep his catheters clean. By that I mean not only to wash them carefully and to oil them carefully, but to use some solution to make those catheters aseptic. I can agree with Professor Chiene that a good many of the cases referred to by other writers and by Sir Andrew Clark are cases of septicæmia caused by the introduction of germs. But I think there is something beyond that,—excluding all those cases of supposed diseased kidneys,—there are cases that have peculiar symptoms, after the introduction of the catheter, that we cannot explain by any of those things mentioned. Those cases are the ones referred to by Sir Andrew Clark in his most admirable paper. I think that the paper has helped to crystallize a great many things that have been floating about in the minds of men ever since Banks's paper. I think we have been careful when introducing the catheter into unknown urethras. I have taught this in my lectures since 1871, and I have even gone the length of having a regular drill for all the cases in which the catheter has to be used—that is to say, if I admit a patient into the wards I do not pass the instrument the day the patient comes in. The patient has his bowels cleared out, gets a dose of quinine shortly before, and is made to go to bed immediately after the catheter is passed. There are, of course, cases coming to the waiting-room that we may never see again; but if I see a man with swollen feet and swollen eyelids, showing that he had kidney disease, then we arrange with him to stay in till the next day. I must say that since that precaution has been taken I have had very few bad cases in connexion with the use of the catheter. I now come to the remark that there is a personal equation—that we have to treat each person separately—and if one is to be a successful surgeon or physician he must remember this personal equation. Some patients stand any number of instruments without bad effect, while others will have very remarkable and rapid symptoms of effects on the nervous system—pyrexia and other serious symptoms ending sometimes in death, or results like those in the case mentioned by Banks, where the patient died in a very few hours. It is very important that we should remember this personal equation of the patient, and try to find out why one man will suffer when another will not. Surgeons know that some have regularly instruments passed, probably go home and get drunk, and come back week after week without experiencing the slightest harm. When we know that a patient is not liable to suffer from catheter fever we can take liberties with him, but until we know him we ought not to do so. We ought to prepare the patient, put him to bed, and treat him as if he were about to undergo a serious operation. I think that is

one of the lessons that we have learned from the most valuable address which we have heard.

Dr Bramwell.—There are one or two things I would like to bring before the notice of the Society, and they mainly, of course, refer to pathological conditions found after death, and the causes which we may presume to be at the bottom of this so-called catheter fever. I in common with most young physicians have not had many opportunities of meeting with these cases; but it so happens that at one time I had charge of a large workhouse hospital, where many cases of enlarged prostate occurred, and I remember cases in which the patients died after the introduction of the catheter. And in more than one case, after making a post-mortem examination, there were found conditions very similar to those recorded in Sir Andrew's case of 1865, where there was a sloughing or diseased condition of the bladder without any very marked alteration in the kidney condition. In common with many others, these cases attracted my attention at the time. I must say that I have not been able to come to the conclusion that the uræmic theory can be so easily disposed of as the previous speakers seem to think. There is, I think, in many of these cases a distinct uræmic element. I base that opinion on certain facts. I think we must admit that in many of the cases in which there has been long-continued over-distension of the bladder,—and these are the cases in which catheter fever chiefly occurs,—there is a distinct structural alteration of the kidney before the catheter was passed. The observations of Mr Berkley Hill and Dr Marcus Beck show that in a very large proportion of cases of enlarged prostate the kidney is diseased.

Sir Andrew Clark.—In a third of the cases exhibited there was no trace of kidney disease at all; and in the cases examined at the hospital in Paris alone about a third were found free. Those old standing cases were found free from all structural alterations in the kidneys. In 26 cases out of 66 no evidence was found of renal disease.

Dr Bramwell.—I will take it, then, that in two-thirds of the cases kidney disease was mentioned in the hospital books. I think that shows that in a considerable number of cases of prostatic enlargement with dilated bladder—I do not say in all—there must be some structural lesion in the kidney before the catheter was passed. Then I say, with Professor Grainger Stewart and others, that in many cases the sudden withdrawal of a large quantity of urine from the bladder produces acute congestive changes. I well remember a case of supposed ovarian tumour which turned out to be a dilated bladder, in which the withdrawal of a large quantity of urine was followed by profuse hæmaturæa. There is a presumption, therefore, that the sudden withdrawal of the urine in cases of enlarged prostate will produce congestive changes in the kidney. In the debate in London Sir Henry Thompson stated that the

dangers of catheter fever to a large extent depend upon the amount of urine which has to be withdrawn, and which remains in the bladder afterwards. Mr Harrison also stated that since he had injected antiseptic solutions into the bladder after withdrawing the urine in cases of enlarged prostate, he had not met with any cases of catheter fever. Possibly one element in the success of that mode of treatment is the prevention of acute congestive changes in the kidney. Then, again, we know that in many cases which die from catheter fever, acute septic or inflammatory changes are found in the kidney after death. The condition of the urine both before and after the passage of the catheter also seems to me to show that some structural alteration in the kidney is often present. In many cases of enlarged prostate, before the passage of the catheter, the urine is too copious, too pale, and of too low specific gravity; and, although such a condition of the urine may depend upon vasomotor changes as well as cirrhosis of the kidney, it shows that the kidney is in many cases inadequate, and unable to cope with any sudden strain which is thrown upon it. In cases of catheter fever the amount of urea, according to Sir Andrew Clark, is diminished.

Sir Andrew Clark.—Sometimes.

Dr Bramwell.—I thought Sir Andrew Clark had said always. But in these cases in which it is diminished there must be, I should think, some change in the kidney to account for the condition, for one of the main characteristics of fever is an increased production and excretion of urea. The septic theory also, I think, is deserving of considerable attention, but as Professor Chiene and Dr Wyllie have spoken on that point, I will not say more. The opinion which I have formed with regard to the causation of death after the introduction of a catheter is, that in some cases it is due to shock, in others to uræmia, in others to septicæmia, but that in the great majority of those cases to which Sir Andrew Clark particularly alludes death is due to a combined condition of uræmia and septicæmia.

Sir Andrew Clark.—I have distinctly stated that there was nothing I have ever seen of a uræmic character in these cases. Perhaps Dr Bramwell will inform us what facts in the cases show that they were uræmic. The evidence was that they were not uræmic, the fever that preceded death had nothing of the ordinary character of uræmia. It will be well if Dr Bramwell will state the facts from which he infers that death was due to uræmia.

Dr Bramwell.—I meant to say that death is sometimes due to uræmia, but that in the majority of cases such as Sir Andrew Clark has described, it is in my opinion impossible to exclude the condition of the kidney. I do not mean to imply that the symptoms are identical with those of ordinary uræmia, but that they are partly uræmic. Sir Andrew Clark has himself stated that the symptoms are partly those of

uræmia, partly those of septicæmia, but not characteristic of either, and that seems to accord with my own view.

Mr John Duncan.—I think that there is very little that can be added to the exhaustive discussion which has been so ably and eloquently introduced by Sir Andrew Clark. The two questions which he introduced to the meeting were—first, Why does a patient die after the introduction of the catheter? and next, What can we do to prevent it? Those seemed to be the chief points. In regard to the first question, “Why does a patient die after the introduction of a catheter?” I should be inclined very strongly to bring all the cases into two categories—one neurotic, and the other inflammatory. That there is a neurotic element which enters into the consideration of these cases there can be no doubt. The extremely intimate connexion of the urinary organs with the nervous system, illustrated by the shudder which sometimes follows the passage of the last drops of urine, explains the rigor beginning almost immediately after the passage of the catheter and death sometimes occurring, as in the case recorded by Dr Banks, within six hours—a period so short that it was impossible that any other cause than a neurotic one could be in operation. That these neurotic causes may come into force in various ways is certain. An interesting case which occurred to myself was one in which the patient had suffered for a long time from rigors which came on after the passage of the catheter and passed away. He at length died in the high pyrexia of one of them, and at the autopsy we found apoplexy in the brain. Such accidents may occur in consequence of this neurotic element. There is no doubt that reflex nerve action may in various ways cause death after the passage of the catheter. All the other causes I would be inclined to class under the head of inflammatory. I am not prepared to deny, but I doubt the cases of symptoms following mere withdrawal of an excessive quantity of urine. I have seen cases in which the pressure, very long continued, has been removed without the smallest evil resulting. One case which is brought to my mind illustrates this point. It was that of a clergyman from the north, who was sent to me by Dr Warburton Begbie, and who had suffered many things at the hands of physicians for many years. He had been treated for polyuria; and diabetes insipidus was the leading diagnosis. Dr Begbie noted that there was something else. With antiseptic precautions and great care I drew off I do not know how many ounces of water. I continued to draw it off daily for three weeks without the slightest injury to the patient, whom I endeavoured to teach the use of the instrument, but never allowing him to do it himself. After that time the patient went home with the catheter. I heard afterwards in a letter that two or three days after he went home his urine became turbid and fever followed, and he died in a few weeks. The removal of undue pressure is not one of the causes likely to occur, but inflammatory affections are. I agree

with Professor Chiene and Dr Wyllie that the causes of inflammation are usually to be found in septicity; but there are other inflammatory affections independent of putrefaction. There was one case that Dr Watson may remember, which occurred when I was a house surgeon in the Infirmary 25 years ago, in which retention of urine from enlarged prostate had taken place, and in which various surgeons had endeavoured to pass an instrument, but without effect. Being of a cautious disposition, I sent for the assistant surgeon in Professor Syme's absence, and Dr Watson succeeded in passing a catheter. The patient died in a very short time, with a prostate in which we found eleven false passages. Such are cases of inflammation resulting in death in consequence of the passage of the catheter. They die of inflammatory fever, sometimes with suppression of urine. But they are rare compared with the septic cases—cases such as that of the very distended bladder in which septic influences were allowed to be introduced. So far as I am aware myself, I should attach more importance to the impurity of the catheter and the introduction of air through the catheter, than to the other two causes to which Professor Chiene has referred. The introduction of air is really important, and especially in hypertrophied bladders which do not readily contract, which have a negative pressure, which, if you squeeze, rebound. There is a considerable tendency in old prostatic cases to this occurrence. And I think that there is another class of septic cases which we must take into account, what Sir Andrew Clark has referred to as those which come from within, where local inflammation is capable of determining to the locality in which the inflammation exists wandering germs in the body. It is possible, and exceedingly likely, that septic infection may thus occur. Death may occur in various ways by inflammation. It may lead to death by inflammatory fever, by septicæmia and pyæmia; and it is possible there are other forms of toxæmia, as gonorrhœa is sometimes followed by rheumatoid affections of the joints. In conclusion, in answering the other question, it is plain, if these remarks are correct, that the points which we have to attend to locally, independent of the general treatment, are the most absolute gentleness in the use of instruments, the use of the very softest form of instruments we can employ, and the greatest possible care to prevent the entrance of septic organisms by the catheter.

Sir Andrew Clark.—I have very little to say in reply, and much more than I can conveniently say in acknowledgment. The only points offered to me for reply are in the very remarkable and very able address of Dr Watson, and in the address, also able and important, of Dr Bramwell. The only point in Dr Watson's address which calls for note is as to the term "catheter fever." I give it up entirely. It was used provisionally and temporarily, and I should be sorry if the name remained with the subject, as it

threatens to do. The contention of Dr Bramwell, if I understand him correctly, is to the effect that in all such cases there is previously existing renal disease.

Dr Bramwell.—In many cases.

Sir Andrew Clark.—I had not understood that. Anyhow, I think that Dr Bramwell regards too lightly the disturbances to which I have alluded in the metabolism of the body. It is one of the leading factors in fatal issues. Another remark of Dr Bramwell's was to the effect that in such cases, particularly cases of enlarged prostate and distended bladder, we could know that the kidneys were diseased by a study of the condition of the urine. That is true and is not here contested, but such cases do not necessarily occur in middle age nor with diseased bladders. During the time that this subject has been under discussion, a case occurred in a young person that came under my notice. Coming from the Clinical Society on the night that I ventured to ask for information on this subject, one of the most distinguished surgeons in London said to me, "It is curious I have never seen a case of death due to the use of the catheter." Within four days from that time this surgeon wrote a note to me saying, that the son of one of my old colleagues was very ill, and his father wished me to see him. He told me that the young man had a slight and recent stricture; that another surgeon had treated the case, but refrained from passing the catheter; that he, desirous of giving the patient relief, passed the catheter, and that in twenty-four hours he became ill. When I looked at the patient I conjectured that the case would end fatally, and in thirteen or fourteen days the patient died. There was no enlarged prostate, no dilated bladder, and the only thing found after death was so-called purulent arthritis, and the commencement of slight catarrhal and interstitial nephritis. The interesting point is not the youth of the patient, but the condition of the urine, which I examined every day, and I make this explanation now to answer Dr Bramwell. In many of these cases, as in the case I am now relating, the urine assumed the ordinary character of a febrile urine. It diminishes in quantity, but rises in density, and deposits lithates. In this case the urine contained over three per cent. of urea, inorganic salts were present in abundance, and urate of soda was found in excess.

Dr Bramwell.—I did not mean to say that in regard to septicæmic cases, or where the kidneys are healthy, but in reference to those chronic cases where the urine is too copious, and where there is a presumption that the kidneys are in an unhealthy condition.

Sir Andrew Clark.—In those cases to which Dr Bramwell now alludes, I have not a complete and confident answer to give. The only answer I can give is this, that there are many cases of persons passing a low density of urine, and when the whole urine of the twenty-four hours is collected, there is found, by Liebig's process, to be very nearly a normal amount of urea; but certainly in other

cases, as Dr Bramwell has stated, there is a deficiency, and it is certainly true that in those cases there is a liability for surgical operations to be followed by disastrous results. This is a point on which Sir James Paget has remarked very fully in his clinical lecture on the various risks of operations, and one upon which I myself have dwelt in relation to the occurrence of diseased states in renal inadequacy. In the way of acknowledgment, I have to say more than I can find time to say. The first acknowledgment I have to make is one of sincere gratitude to you, Mr President, and to the Society, for the kindly and friendly manner in which you have been pleased to receive my imperfect communication. The next acknowledgment I have to make is to thank the members of this most important Society for the great advance which has, I think, been made to-night in the discussion of the subject. Even to myself, who have thought so much about it, the subject has assumed truer proportions and a clearer position. I am sure that the discussion will exert an important influence on the profession at large, and that it will no longer be open for anyone to say that as a body we are unacquainted with what the leaders of it have long known so well.

The President, addressing Sir Andrew Clark.—I have now only to thank you in the name of the Society for having delivered so able an address to us, and also to express the hope that, as a distinguished Scotchman, you will return more quickly to the north, and give us soon again the benefit of your professional experience.

Meeting V.—March 5, 1884.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

I. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Mr A. G. Miller* exhibited (*a*) an ENLARGED BURSA PATELLÆ; and (*b*) a CAST OF THE KNEE taken in plaster and varnished with paraffin to show the appearance before the removal of the bursa. The specimen was interesting not so much from its rarity, as from the peculiar shape of the tumour which was removed. It presented a conical outline above the patella. It was very firm, almost cartilaginous in hardness, and on removal was found to be a very much thickened bursa filled with fibrous material. It appeared to be almost solid. There were also present in it a few of melon-seed like bodies.

II. EXHIBITION OF INSTRUMENTS.

1. *Dr Roderick Maclaren* of Carlisle showed (*a*) a DRILL. He

did not claim any originality for it; but it was an instrument which he had never seen in any surgical instrument-maker's stock, and he thought it might be of use to surgeons, more particularly in removing portions of the lower jaw. (b) A BONE-CUTTER worked by screw power. The ordinary bone-pliers were often deficient in power and required the exercise of a great deal of strength on the part of the surgeon. It had occurred to him that screw power might be utilised for this purpose, and he had got an instrument made by a local engineer, by which the bone was cut between a fixed blade and a chisel driven by a screw.

III. ORIGINAL COMMUNICATIONS.

1. FIVE CASES OF AMPUTATION AT THE HIP-JOINT.

By RODERICK MACLAREN, M.D., Senior Surgeon to the Cumberland Infirmary, Carlisle.

THE cases were all treated in the Cumberland Infirmary.

CASE I.—N. R., male, *æt.* 10, admitted 2nd April 1880. Had acute necrosis of the femur of three weeks' duration. Amputation seven weeks after admission, the delay due to parents refusing consent. Temporary improvement for a week; then pelvic abscess, necrosis of ilium, and death from exhaustion twenty-six days after the operation.

CASE II.—A. P., male, *æt.* 17, admitted 24th September 1879. Had necrosis of the lower third of the femur of eight months' duration. Sequestrum removed. This followed by progressive suppurative periostitis, frequent attacks of pyrexia, and general exhaustion. Amputation eight months after admission. Good recovery. Is now well and vigorous.

CASE III.—J. L., male, *æt.* 20, admitted 9th June 1880.—Hip-joint disease of six years' standing. Much suppuration, consequently extreme weakness. Amputation five days after admission. Slow recovery. Subsequent removal of tuberosity and ramus of ischium and much bone from the neighbourhood of the acetabulum. Now well, and works regularly at his trade as a watchmaker.

CASE IV.—T. B., male, *æt.* 6, admitted 21st June 1882. Acute necrosis of femur of four months' duration. Much discharge of pus; boy very thin. Amputation five days after admission. Rapid recovery; stump healed in five weeks. Some bone reformed in stump. Is now in vigorous health.

CASE V.—M. A. S., female, *æt.* 12, admitted 20th June 1883. Hip-joint disease of more than three years' standing. Excision of hip-joint May 1880. This followed by increase of local bone disease, fresh abscesses, and general deterioration of health. Patient

sent to Home for Incurables ; condition believed to be hopeless. When there, enlargement of liver and albuminuria ; subsequent disappearance of these conditions. Readmitted to infirmary in an exceedingly feeble and emaciated condition, with the hip discharging largely. Amputation 10th July 1883. Slow but steady progress to recovery. Still unhealed sinuses.

The method of operation in all these cases was that of Mr Furneaux Jordan, which Dr Maclaren performs by making a circular incision or double flaps somewhat above the middle of the thigh, then an incision along the outer side of the femur, ending above the trochanter, which allows the disarticulation of the head of the bone, the important point being the low division of the soft parts. For controlling hæmorrhage he prefers Davy's lever to any other method. But, as it is sometimes difficult to place the lever on the proper spot, and as it would in some cases be dangerous, on account of emaciation, compression by a rubber bandage was also spoken of as a convenient and effective way to stop the circulation. In the above cases, after removal of the limb, the principal vessels were tied, the lower edges of the wound sutured, and then the cavity stuffed with sponges wrung out of carbolic lotion, and wrapped in gauze ; this saved the delay which would have occurred if every little bleeding point had been tied. The sponges were generally removed the day after the operation. Case II. was operated on under spray, and dressed with gauze dressings. The others were dressed in the simplest possible manner, mostly laid on a pad of oakum and covered with a layer of lint soaked in carbolic oil. In the last two cases the cavities were packed with carbolized lint into the acetabulum. Movement was avoided as much as possible, and the wounds frequently washed and syringed with carbolic lotion. The resulting stumps were all good and comfortable.

The low method of amputation can probably never be employed in cases of accident ; any injury which would necessitate disarticulation would injure the soft parts too high up. But for disease the advantages are,—“ much less shock, less of the body is removed, the nerves and vessels are divided further from the trunk, where they are smaller ; less hæmorrhage, the vessels divided are all branches of the femorals, though the anastomosis of the branches of the deep femoral, with branches of the internal iliac, makes it advisable to control the circulation in the common iliac during the operation ; there are certainly fewer vessels to tie, and these smaller ; the form of the flaps gives better drainage than the old method, in which the posterior flap formed a cup where discharges collected.” “ The results of a comparison of the two operations on the dead body were as follows:—A limb can be removed by either, without any hurry, in three quarters of a minute. The flap operation takes eleven seconds less than the Furneaux

Jordan one; the length of the edges of the skin incisions in the former was 29 inches, in the latter 26 inches. The area exposed by the flaps was 40 square inches, by the other 64 square inches." The mortality of the flap operation for disease has been 40 to 50 per cent. Dr Maclaren collected from various sources 25 cases of the low amputation, of which 5 were fatal within a short time of operation, and 1 three months afterwards from visceral disease, giving a mortality of 20 or 24 per cent. This will probably be improved upon as the ideas of surgeons become more definite in regard to the conditions which justify the operation. These are,—acute necrosis of the head and any considerable portion of the shaft of the femur; extensive disease of the head of the femur and also of the pelvis; old-standing hip disease, with great exhaustion from long-continued discharge; when excision has failed; and, lastly, malignant disease of the femur low down.

Amputation after excision should be an infrequent occurrence. And it would be, if extensive pelvic disease or much general exhaustion were looked upon as contra-indications to excision. Amputation has the following advantages for recovery over excision:—It removes at once at least half the disease, it avoids the risk of pus collecting in pockets and infiltrating the muscular planes, the after-treatment is much less painful, and a larger blood-supply, and consequently a greater power of overcoming disease, is directed to the pelvis and its surroundings.

The President said he was certain the Society felt much indebted to Dr Maclaren for reading before them his interesting paper, and for coming such a distance to give the details of these valuable cases.

Prof. Annandale said the first remark he would like to make was regarding the disease for which this operation by Furneaux Jordan's method was most frequently done. By far the larger number of the cases on the list Dr Maclaren had drawn up were cases of hip-disease. This impressed him still more with what he had already brought before the Society on more than one occasion, viz., the importance of early diagnosing cases of hip-disease in which there is suppuration and disease in the head or neck of the bone. In his experience, if excision was early performed in such cases, it prevented extension of the disease to the acetabulum and pelvis, where, as a rule in most cases, the disease was secondary. It so happened that on several occasions in making an exploratory incision he had found the disease very limited, sometimes affecting the trochanter alone, sometimes a necrosis of the neck of the femur. By removing this disease the result had been most satisfactory. The patient, where such had been done, would recover with a limb shortened, but with a freely movable joint. The second remark he would make was that all surgeons were agreed that this method of amputating

must be considered as a much safer method and attended with less risk than the ordinary hip-joint amputation. All who had experience of it must have found that shock was much less than in the ordinary amputation. There was one little hint he had learned in practising this operation, and that was, that instead of first making the circular and the perpendicular incision, and afterwards disarticulating, it was more advantageous to saw through the bone at the situation of the circular incision, and then to dissect it out and disarticulate. He believed this had been suggested by some one, but he did not know of it till he had practised it for himself. Another modification of the hip-joint amputation had recently occurred to him. They were all agreed that the prime risk of such an amputation was shock from removing such a large portion of the body, and it might be also from bleeding, if precautions were not taken to prevent that. The point that had often struck him in connexion with shock from such an amputation was that they opened into a large joint. He had also found in the after treatment of his cases that the cavity of the acetabulum had been a difficulty as regards drainage. Fluids, blood, serum, or pus had sometimes accumulated there, and it had not been easy to drain them. It had struck him that in some cases where it was not necessary to disarticulate, they might perform an operation which might carry out all the principles of amputating at the hip-joint. Take, for instance, the case which led him to think of this, a tumour originating in the soft textures of the thigh, passing up and over-lapping the hip-joint. Amputation according to the ordinary method would have been impossible. It could only have been done by dissecting up a skin flap and performing disarticulation. This was a case in which it was not absolutely necessary to disarticulate, the bone not having been originally affected. It occurred to him that it would be sufficient if a sub-cutaneous section of the femur, immediately below the trochanter, after Adam's method, were first done, the soft parts afterwards divided, and the limb removed. He thought this method might be applicable in certain cases of injury. His last observation would be in regard to the arrest of hæmorrhage. He had not been successful in the use of Davy's lever. That might be due to his own clumsiness. He preferred the abdominal tourniquet, but he should be inclined to try first Esmarch's method.

Mr John Duncan supposed it was some ten or fifteen years ago, when he was assistant-surgeon and in charge of Mr Spence's wards, that a case was brought in requiring amputation at the hip-joint. In amputating he used the abdominal tourniquet. Notwithstanding, a considerable amount of blood was lost at the operation, and the patient was in the most imminent danger of life from embarrassment of respiration produced by the tourniquet. He determined to do his next case in a different way. Very shortly afterwards another patient was brought in requiring a similar

amputation. This was performed by the method described that evening, and the hæmorrhage controlled (as had also been described) by an Esmarch's band round the groin, kept in place by bandages. The circular incision was first made, the bone sawn through, the bloodvessels tied, and the head of the bone disarticulated by the perpendicular incision. A hip could be amputated in that way without Davy's lever and without abdominal tourniquet, and not a drop of blood lost. He thought he had made a good discovery of a new method of amputation. Besides the saving of blood he thought it might be useful in other ways,—for example, rendering the chances of keeping the wound aseptic greater, by being further from the anus. He thought of publishing the method, but on looking up the literature he found it was no new thing, but an old operation described by Dupuytren. He had no doubt it had not come into general use because the rapidity of the flap operation made it the favourite in the pre-chloroform days, when rapidity was so essential.

Mr Joseph Bell said he thought that Dr Maclaren must be gratified to find that the consensus of surgeons was that Furneaux Jordan's method of amputation was the safest and best. He was very much pleased with the clear and brief manner in which the cases that had been brought before them were detailed. It so happened that he once talked over this operation with a greater than Dupuytren, their old master Syme, who told him that this method, now known as Jordan's, was the method which ought to be adopted, and he would do it the first time he got a chance. He never got a chance. He had strong objections to the abdominal tourniquet, and prophesied that surgeons would soon be doing the operation in the way that had been described. He (Mr Bell) did not think that it could be done in many primary cases. Mr Annandale's plan for amputating subcutaneously was very ingenious and might be useful, but he thought there were but few cases where it would prove applicable.

Dr Bramwell thought that a very interesting point in one of the cases was the disappearance of the waxy degeneration. He had not seen a case in which this had undoubtedly occurred, but there was no reason why a patient with waxy disease might not get perfectly well. Dr Maclaren's case differed from any he had ever heard of, inasmuch as the waxy disease, as evidenced by the enlarged liver, albuminuria, and diarrhœa, disappeared when the suppurative process was still going on. This could but rarely occur, and was a point worth noticing.

Mr A. G. Miller would like to make a remark on two points. His experience was of one operation done in the manner described by Mr Duncan and Mr Annandale, first, a rapid circular incision and division of the bone by the saw, followed by arrest of hæmorrhage and disarticulation of the head of the bone through the perpendicular incision. The patient, a boy, lost scarcely any

blood, the common femoral having been compressed by Dr P. H. MacLaren. No clamp, lever, or tourniquet of any kind was employed. With regard to sawing the bone through before disarticulating, he was not inclined to agree with Mr Annandale, though it might seem presumptuous of him to say so, that this mode of procedure was an advantage. In his case he experienced considerable difficulty in disarticulating from the want of leverage. He was anxious not to leave periosteum, as he thought it might be leaving a nidus for disease. It was very difficult to remove the loose periosteum without the leverage to help him. The boy came back with a slight reproduction of caries in new bone, which had grown from a little periosteum he had left. There was no doubt about this, because the bone had the peculiar character of periosteal bone, being in little nodules. The operation was done for advanced hip-disease. There was great deterioration of the bone, the cancellous bone being oily, and the laminated almost a mere shell.

Dr Heron Watson rose at that late period of the discussion rather for the purpose of adding his voice to the general consensus of opinion regarding the admirable paper which had been read before them. The previous speakers had left but little to say on the subject, but there were one or two points on which he should like to add a few words. Long before he was aware of any description of the method of amputation now known as *Furueux Jordan's*, it was brought under his notice by a case in which, during operation for excision of the hip, the too energetic assistant thrust the bone out from the periosteum. Amputation was at once performed by a circular incision at the lower end of the perpendicular wound. In another case where there was penetrating disease of the acetabulum, as well as other disease, a straight incision was made in the line of the shaft of the femur to about half-way down the thigh, and the head of the bone being turned out, the soft parts were cut through circularly. Had those cases been published at the time, he had no doubt they would have been regarded as an innovation in surgery; but he observed that the same operation had been described not only by *Dupuytren*, but by an earlier surgeon, *Ravaton*, in 1743, and it was not improbable, that if the records of surgery were searched, it would be found that it had been practised at a still earlier date. As to the mode of procedure, he was in favour of making the perpendicular incision first, then turning out the head of the bone and making the circular incision. In this way hæmorrhage might be readily prevented without the use of abdominal tourniquet or other methods of compressing the aorta or common iliac. What ought to be done when, after excision or amputation, an abscess within the pelvis with a small aperture through the acetabulum was found? These cases were not so rare as had been said. He suspected that in many cases the disease commenced in the acetabulum and not in the head of the femur. That was not an unlikely spot for disease when they

remembered that it was more exposed as a tripartite centre of growth to the chance of those trivial injuries that lit up disease than was even the head of the bone. Gouging out the affected part of the acetabulum was not sufficient. Sawing the entero-acetabulum out was better. It was better still, where the internal abscess was large, in addition to the cutting out of the acetabulum, to make an incision down upon the ramus of the ischium, and pushing a pair of dressing forceps up along the line of the bone into the abscess sac, establish a distinct drainage-hole to allow of the operation wound healing. With reference to Dr Bramwell's remark regarding the disappearance of waxy disease while suppuration was going on, he could give his testimony that such things did occur. He was of opinion, however, that in many of the cases, the waxy disease was due more to the too diligent administration of alcoholic stimulants by the friends than to the long-continuance of suppuration.

Dr Maclaren thanked the Society for the patience with which it had listened to his paper, and especially those members who had made remarks upon it. It had been a great pleasure to him to have raised and listened to such a discussion. He had not attempted to lay claim to any originality in the paper, and he found he had brought before them no new thing. The operation, he had learned, dated back to the early part of the 18th century at least. With the remarks of Mr Annandale on excision he was very pleased to be able to agree. It was a matter of consequence to do the operation at an early period. His first cases were very unsuccessful. For a time he gave up the operation; but thorough drainage of the acetabulum and division of the bone *in situ* had this result—that of his last four cases two were now healed and well, the other two steadily progressing to recovery. From the remarks made by different speakers he observed that he had omitted reference in his paper to shock. He should have stated that there was a remarkable absence of shock, a very different thing from what was seen after a flap operation. As to division of the bone and removal of the head, it seemed to him that the extra time it required was an objection to it. In one of his cases, the second, he did divide the bone at the upper third, then, finding the portion left affected, he removed it. It did not give him much trouble, but he could readily conceive of its doing so, and it certainly caused much delay. As to Davy's lever, it was easier to apply on the left side than on the right. The subcutaneous method of Mr Annandale was ingenious, but he fancied there were not many cases in which it could be done. The origin of the disease, his own experience led him to believe, was more often found in the head of the femur than in the acetabulum. As to Dr Watson's point about the waxy disease, he was inclined to accept it as true. The patient in whom it occurred was the pet of an incurable's home, and probably had been fully supplied with wine and delicacies of a like nature.

2. WOUND TREATMENT WITHOUT THE SPRAY.

By JOHN DUNCAN, M.A., M.D., F.R.C.S. Ed., Surgeon to the Royal Infirmary, Edinburgh, and Lecturer on Surgery, Edinburgh School of Medicine, &c.

Two years ago I brought before this Society what I thought to be strong arguments in favour of the substitution of cotton wool impregnated with a non-volatile antiseptic for the carbolic gauze commonly in use in the treatment of recent wounds. A year ago I related also to this Society a series of experiments which led me to believe that the spray of carbolic acid was useful only in so far as it threw a germicide solution on a wound, that it had no effect whatever in rendering the atmosphere aseptic. As, therefore, the spray is not only inconvenient but also useless, it has been entirely abandoned in the wards under my care, and I desire to-night to lay before you a record of my surgical experience since I carried these changes into effect.

But first I wish to make a retraction. In one of these papers I spoke, although guardedly, of the possibility of giving up also the use of drainage-tubes, and I mentioned instances of amputation below the knee, at the ankle, and in the forearm, besides one or two other operations in which this had been done with complete success. I have since had further experience of a similar kind. Take one case out of several. A short time ago a girl was brought to the Infirmary suffering from compound fracture sustained in a steam laundry. I amputated the forearm a short distance below the elbow, as well as the tips of three fingers on the other hand. On the eighth day she was out of bed, and the dressings being removed at the end of a fortnight, the wounds were found to be healed. Nothing can be more satisfactory than to lay a patient thus aside, and simply watch the pulse and the temperature chart till sufficient time has elapsed for the wound to heal. I thought and still think that under certain conditions this might be relied upon. The first of these conditions is that the wound be dry. The surgeon cannot spend too much time in arresting the flow of blood. Blood mechanically separates the parts to be united, and removes accidental microzymes from the germicide action of the tissues. It is better to have the wound a mass of ligatured points than to have the least flow of blood from the smallest vessel. Next it is a condition of success that by pressure or position the surfaces of the wound be kept in absolute and steady apposition. Lastly, it is essential that applications to the wound be free from irritating properties. Unfortunately we have not yet attained perfection in these necessary conditions. A little reactionary hæmorrhage will now and again occur, however careful the surgeon may be. It is impossible to make accurate and effective apposition in every form of wound. We do not yet know any bland antiseptics.

I had recently a case which is so curious that, while citing it as

an example of the evils which accrue from want of drainage, I should like to record it on its merits.

A man, æt. 46, was sent to the Infirmary from the New Town Dispensary with a tumour in each groin. For twenty years a swelling had existed on the left side, which could be made to disappear on pressure. Eight weeks before admission it had rapidly increased and hardened, while the skin over it became red and adherent. I cut down, 12th February 1884, upon this tumour, and excised first a large mass of inflamed glands, each with its core of pus. Partly concealed by the glands I found a fatty tumour, and in dissecting it out came to a sac emerging with the fat from the widened crural canal. The tumour on the right side proved to be purely fatty, and its neck also passed through the crural ring. By dragging upon it, a portion of peritoneum could with some difficulty be brought into view. They were examples of that hernia of hypertrophied subperitoneal fat, which has been described, chiefly by German authors, as bringing about hernia of the peritoneum also, and which in this case had produced on one side a well-marked lymphatic inflammation. The bleeding being carefully arrested, the wounds were dressed without drainage. On the right side healing took place by first intention. On the left slight oozing distended the parts, diffused itself into the surrounding cellular tissue, and caused sloughing of the thin and inflamed skin which had been dissected from the glands. The wound remained aseptic, but a slow cicatrization was the result in a case where effective drainage would have insured a speedy cure.

Yet again, on 5th Feb. 1884, I excised the wrist, in a girl æt. 15, for fungous arthritis. The skin was yet unbroken, and I used bichloride of mercury as the antiseptic. When sewed up it was absolutely dry. The temperature next day rose to $101^{\circ}5$. If in any case the temperature reach 101° , I regard it as an indication that the wound should be examined. It has become septic, or there is malposition, or tension, or some other local or general disorder. In this case I found the wound tense and red, distended by a dessert spoonful of bloody serum. A drainage-tube was introduced, the temperature fell at once, and the case required no further care.

These were exceptions no doubt, but they have been sufficient to induce me to resume the habitual use of the drainage-tube in large or deep wounds. That necessitates a dressing for its removal, but this inconvenience is counterbalanced by the avoidance of accidents. I am yet undecided when this dressing ought to be performed. Twenty-four hours is sufficient for drainage in aseptic cases, and I am inclined to think that it is best then to remove the tube. If you leave it for two or three weeks there sometimes comes, in strumous patients, a tedious sinus, although with them, immediately after a wound, separation seems as active as in the most healthy and robust. I think it not impossible that we may arrive at a drainage-tube of absorbable material which may meet our

wants. Those at present in use last too long and needlessly delay the complete closure of the wound. My house-surgeon, Mr Wilson, has devised a tube of plaited catgut, which I think will meet our requirements. It can be made of any thickness or thinness, of material which will quickly disappear or of lasting character. If made with a large lumen it permits the escape of fluids as easily as an india-rubber tube of the same calibre, and for the same reason and because of its loose texture the granulations eat it up more easily and quickly than an ordinary skene of catgut or a decalcified bone. It will not last sufficiently to serve in a septic case. Then we must use india-rubber or glass.

I have wished to make these observations because I formerly left too much out of account the rare cases in which evil arises from want of drainage, and because, of course, the result of any method of wound treatment must be thereby modified.

But to return to our subject. The place to be held by the various germicides in the treatment of septic wounds is a question of great interest, and which has been by no means decided. Which is best for this purpose? In what form is it to be applied? Is the application to be constant or intermittent? Are they to be used with a curative or palliative object in view? Is the possibility of further contamination to be prevented by dressings? These and a thousand other questions are constantly pressing, and have not yet been adequately considered. I hope to return to them. Meantime, I confine myself in this paper to the treatment of recent wounds.

I need not recapitulate here the arguments which led me to prefer a dry absorbent and elastic dressing, which retains its antiseptic properties for an indefinite period. Nor need I detain you with a *résumé* of laboratory experiments, which went to show that the spray, as spray, was not germicide. I take these for granted.

Fortunately, during an operation a wound becomes contaminated far more frequently by contact than through the air. The surgeon's fingers, instruments, coat sleeves, blankets, towels, sponges, such are the sources of putrefactive mischief.

First, then, it is essential that whatever may come in contact with the wound should be absolutely pure. With the necessary precautions for this purpose surgeons are familiar, as well as with the importance of attention to the minutest detail. What is needed in the laboratory to keep a beaker of fluid or a gelatine dish aseptic, is needed also for a wound. A little matter, but not without its importance is the surgeon's comfort in this respect. Strong solutions of antiseptics injure the skin of the hands and interfere with the delicacy of touch. I have, on the whole, found it most satisfactory to wash first with a solution of carbonate of soda, and then smear the hands with a solution of carbolic acid in glycerine. The glycerine preserves the skin soft and pliable.

But given the well-known precautions as to hands, instru-

ments, patient's skin, etc., we have also to consider how best to protect the wound from atmospheric contamination.

This resolves itself into two periods, viz., during and after the operation.

There is reason to believe that during the operation the chances of contamination are comparatively small. On a surface, three inches in diameter, there will sometimes no germ light in fifteen minutes, and to be certain of a successful cultivation about twenty minutes' exposure of such a surface is necessary. Now, healthy tissue forms, in some respects, an unsuitable soil for the growth of organisms. I have often thought that we might regard the constituents of the human body much in the same light as Darwin regarded living things in general. There is a survival of the fittest, a choking off of the weakest. Every tissue is always trying to enlarge its boundaries, and their territories are settled by a true balance of power. Disturb the balance by strengthening one, or weakening another, and the territorial limit is altered accordingly. The history of tumour growths, of grafting, whether of skin or bone or malignant disease, is an illustration of the law. The eating up of dead bone, of blood, of catgut, of sponge, is yet another example. And so, whether or not, when microbia are brought in contact with the interior of the human body, they will increase or die, must depend on their relative vital power. If, on the one hand, they are very numerous or powerfully vitalized, or if, on the other, the tissues with which they struggle are naturally weak or artificially devitalized, the microzymes will propagate themselves. Otherwise they will not. Were it not for this, there would in the old days have been no possibility of healing by the first intention, and yet it was common enough when the wound was small, and not rare even when it was large.

The tissues, then, are germicide. Is it possible to trust to this in our ordinary surgical practice? It is proved by the experience of many surgeons that we may do so in abdominal surgery. A woman, *æ*t. 21, was sent to me by Dr Angus Macdonald, suffering from pyo-nephritis. The tumour had begun eight months before, inflammation spreading up from the bladder during the last weeks of a pregnancy. It was now of large size, reaching past the middle line of the abdomen. I operated by the median incision, and had no difficulty in removing the enlarged kidney. No antiseptics whatever were used to protect the wound during the operation, although, of course, the most scrupulous care in dressing and in all other respects was observed. It is certain that sepsis did not occur, because a glass drainage tube was kept in for three days, and the teaspoonful of serum, which was twice a day sucked up through it, was always carefully examined. The wound was healed in a fortnight, and the patient was out of bed in three weeks. The sequel of the case is so interesting

that it seems worthy of a digression. At the time of the operation I tied the greatly thickened ureter in two places, and divided it low down in the abdomen, between the ligatures. A few drops of pus exuded, which were carefully mopped up, and the cut end touched with carbolic glycerine. Had I correctly estimated the meaning of these drops of pus, viz., that there was communication between the ureter and the pelvis of the kidney, and had I borne in mind that for weeks the urine had been perfectly free from pus, although in the early days of the disease it had been highly purulent, I should have understood that obstruction of the ureter existed below the spot at which I had divided it. The result was that in the fourth week an abscess slowly enlarged the stump of the ureter, and, although we succeeded in opening it from the vagina, it subsequently formed a communication with the bowel, and the patient died exhausted seven weeks after the operation.

But to return, the surgery of the abdomen shows that although it is certain, from the long continuance of the operation, that germs from the atmosphere must obtain access to the wound, they will not propagate themselves unless they be of peculiar virulence and in unusual numbers. I suppose that no one proposes that in the abdomen we should wash the parts with any antiseptic whatever. The certainty of producing inflammation and the risk of poisoning the patient forbid it. Some surgeons still maintain a fond belief in the spray, and cover the abdomen with a mist of carbolic solution. But their results are not better than those of others who do not. Here, then, in abdominal surgery we have facts which inevitably lead to the conclusion that the use of antiseptics, for the purpose of destroying germs in the atmosphere, or even sometimes after they have lit in the wound, is unnecessary, ineffectual, and even dangerous.

May we then argue from this to ordinary surgical wounds? Is there a difference so great between the surgery of the abdomen and that of the rest of the body that a diverse practice should be followed in the one and in the other? It appears to me that this point is still undecided. I have abandoned the spray in all cases, with what results this paper is designed to show. It is useless and inconvenient. May we also abandon the washing of the wound with an antiseptic? I believe that in the great majority of cases no evil would follow. But I have no doubt also that a certain percentage would become septic which would not otherwise do so. That is the case in the abdomen, spray or no spray. Every now and again the ovariologist has to fight septic inflammation and septic poisoning by drainage, and suction, and ice-caps, and other things. And so it is in general surgery, with a certain additional disadvantage in the fact, that the whole surface of the wound is an oozing surface, and that the soil is therefore more suitable for the development of germs.

I cannot, therefore, as yet abandon altogether the application of germicides to the wound. I try to do so now and again in the more simple wounds, and I have not had reason to regret it. I think it may come that we shall operate in an atmosphere rendered sufficiently pure to enable us to do so safely. Meantime, not yet venturing altogether to abandon, we try to moderate the irritating qualities of the antiseptic. With this object in view, I have elected to smear my wounds with a solution of carbolic acid in glycerine. There are two solvents of carbolic acid which mitigate its caustic action—oil and glycerine. After trying the mixture of oil and carbolic acid, I came to the conclusion that the glycerine was in many respects to be preferred. As every one knows, glycerine is itself little prone to putrefaction, and although there are rare organisms which find it a suitable soil, animal substances may be kept in it without decomposing. Oil, on the other hand, parting quickly with the acid, may easily become rancid. Again, glycerine is mixable with water, an advantage of no slight practical importance. Its adhesive property, moreover, at once maintains it on the surface of the wound, and is calculated to catch and retain any organisms which may alight, and so prevent them from easily finding their way into the recesses. I have found that glycerine alone is sufficient in minor wounds, but as a rule, and to make assurance doubly sure, I mix it with carbolic acid. I have generally used one part of carbolic acid to twelve of glycerine, but a solution of one to twenty is, at the same time, perfectly antiseptic, and even more free from caustic and irritating properties. I expected that the attraction of glycerine for water would produce a very copious flow of serum from the wound, but it has turned out that there is not so great an exudation as when the watery solution of carbolic acid has been used. For ordinary wounds it is quite sufficient, the skin having been rendered aseptic, to smear the solution over the surface at the end of the operation, but in wounds in which cavities are laid open, in which there are many recesses, or in which the depth is great as compared with the breadth, it is better to pour on a little from time to time during the operation. If the operation be rendered bloodless, the cavity of the wound may be sometimes filled with glycerine, and the operation be yet carried on with perfect ease under the translucent fluid. With all antiseptics it is essential that they be not introduced under pressure, especially if the tissues be already bruised. I shall presently show that even with glycerine this may prove injurious. If the case remain aseptic it causes a painful cellulitis, if it become septic it inevitably leads to sloughing. It is less injurious, however, than either chloride of zinc or the watery solution of carbolic acid under similar circumstances.

Let me enumerate, then, shortly the modifications of the Listerian precautions which I am in the habit of using. Those which I do not mention I adopt as they are usually carried out. It is well to

wash the hands and the skin of the part to be operated on with a solution of carbonate of soda, and then smear thoroughly with the glycerine solution. After the operation, and during it if it be prolonged or complicated, the wound is also covered with the glycerine. Oiled silk, simple or dextrinised (it matters little which), is soaked in carbolic glycerine, and laid on the closed wound. The object of this is chiefly to prevent adhesion of the outer dressings. The whole is then covered with a large quantity of salicylic wool, a freshly torn surface being placed next the skin. If, on account of oozing, the dressing requires renewal next day, the drainage-tube is then removed; if not, its removal may be delayed for ten days or a fortnight.

I have now pursued this method for nearly a year. It has been modified at different times by using other antiseptics, as bichloride of mercury, thymol, eucalyptol, and others, but I have not been able to assure myself that any one is less irritating or more efficacious as a germicide.

Statistics have, no doubt, a certain value in determining the usefulness of different methods of dressing. But the conditions are so complicated that the record of individual cases is undoubtedly of greater importance.

In the first place, then, I purpose placing before you evidence that the method is effective in the various classes of cases which antiseptic surgery has more especially rendered feasible and safe.

Take first examples of simple osteotomy. Peter Horner, *æt.* 25, came to the Infirmary on 26th June 1883, on account of knock-knee. Eighteen months before admission he had been operated upon in an English provincial hospital, by removal of a wedge of bone from the inner condyle of the right femur. There was now a sinus which had not healed since the operation, and which led to carious bone. The limb operated on was inverted and bowed, the knee and ankle-joints were stiff, and the foot rigidly pointed. An advantage of the carbolic glycerine to which I have not yet referred is its power to correct septicity. I look upon it in this respect as superior to chloride of zinc, and much less irritating if it fail to render the part at once aseptic. In this case the carious bone having been freely exposed was gouged out, and the part being drenched with glycerine at each dressing speedily became aseptic, and after a few weeks healed. The left leg which had been left untouched was now operated upon. It appears to me certain that Mr M'Ewen's plan of operation for knock-knee is greatly to be preferred to all its numerous rivals in respect of simplicity, safety, and the subsequent comeliness and usefulness of the limb. The only modification of it which is an improvement is the placing of the incision on the outer instead of the inner side of the limb. If the wound be over the outer condyle, pressure may be applied on the inner side to keep the limb straight, and much

less chiselling and force is required to divide the bone. It may be objected that a gap is left at the outer edge of the divided bone. The answer is, that such is the case to a certain extent with both methods, that the union of the fracture never fails, and that what slight lengthening there may be of the limb is not inconvenient. By this plan, then, I operated on Peter Horner. The wound was dressed as has been described, and the limb was placed at rest. I am in the habit of using in knock-knee an apparatus which I find useful both for rectifying the deformity without operation, and also after section of the bone. The double long splint is used with the tie rod at the foot, and extension is applied to the limb by weight and pulley. This last aids in diminishing the pressure on the outer condyle, and so allowing its more rapid growth in the child. In the adult after section it tends to steady, wonderfully mitigates the pain of any slight movement, and helps to maintain the proper straightness. But the chief difficulty in the treatment of knock-knee by apparatus is to keep the leg in apposition to the splint. By the slightest eversion the limb eludes the straightening force whether that be rigid or elastic. A little experiment will at once show how this may be avoided. If a long splint be applied to the outer side of a knock-knee, and pressure made to draw the knee to the splint, it will be found that this is easily accomplished, but only because the knee has become slightly everted and flexed, while the heel has separated from the splint. If the heel be held firmly applied to the splint it is impossible to make the knee touch it. I therefore fix the heel by attaching to the lower part of the long splint a little suitably padded wooden trough.

In Peter Horner's case it was impossible to use the double splint, because of the distortion of the right leg. In other respects the apparatus was applied as described. At the end of a fortnight the wound, which had been left open without a drainage-tube, was found to be healed when looked at for the first time. A jointed support was subsequently fitted to his left leg, while a high-heeled boot was adjusted to his right, and he went out after fourteen weeks, walking well with the aid of a stick, a feat which had long been impossible to him.

I have selected this case for narration simply because of its intrinsic interest, not because it presented any peculiarity in regard to its successful result. All the cases of a similar character have done equally well, viz., two other operations for knock-knee, two of section of the tibia for deformed union of old fracture, one of resection for overgrowth of bone in a stump of the upper arm. They were all healed when the first dressing was removed.

Wounds of joints are important from a septic point of view. If they go wrong, the joint is spoiled and the patient in great risk. I have operated once for badly united fracture of the patella of old standing since I abandoned the spray. Considering the good

results that are easily to be obtained by other treatment, and the evil effects that may follow the slightest error on the part of the surgeon or carelessness on the part of the patient, I think it unjustifiable to suture in recent fracture. When the limb is rendered useless by wide separation of the fragments it is far otherwise. The operation, however, is then difficult. In this case, by section of the quadriceps extensor, I was able to reduce the gap from $2\frac{1}{2}$ inches to half an inch. The wound was dressed the next day on account of serous oozing, when the drainage-tube (inserted in front only) was removed. It was not again seen till it was healed. Of the same character is a case in which I incised the elbow on account of synovitis just become purulent. The temperature immediately fell, the pain disappeared, and the only dressing required was for removal of the drainage-tube a fortnight after the operation.

A very good illustration of another class, that of compound fracture, is found in one of the fibula opening into the ankle-joint. The tibial malleolus was also broken. My house-surgeon, Mr Wilson, therefore made an opening on the inner side, passed a drainage-tube through the ankle, washed out with carbolic glycerine, and dressed with salicylic wool. I removed the tube next day, the retentive apparatus was kept on for three weeks, the dressings for a week longer, and the patient left in six weeks with a perfectly movable joint.

A class of case which tests the value of any proceeding for maintaining a part aseptic is the opening of chronic abscesses. A psoas abscess, from caries of the spine, was recently brought to a successful termination after about six or seven dressings at long intervals, and the efficacy of the plan is all the better illustrated that the treatment was carried out by Dr Hodsdon during my absence from town in the autumn. There are at present in the Infirmary two cases of empyæma. In both the treatment has been, so far as it has gone, absolutely successful. The wounds are still open and dressed at intervals, but the temperature is normal, and the pus scanty, pure, and sweet. In such cases the patient is directed to hold the breath while the dressing is changed. Formerly, while I had faith in the-spray, I used often to allow the air to whistle in and out of the opening for considerable periods, while examining the patient or demonstrating to students. Most of my cases became septic, with increase of temperature and pus formation, as one would now anticipate, considering the inefficacy of carbolic spray as a germicide.

I cannot conclude this list of cases without referring to one of which, doubtless, the fatal termination is not far off, but which illustrates the safety of the method. The patient, a young woman of 26, had suffered for a year from sarcoma of the skull. It was nearly two inches in diameter, and so round and well defined that by some who saw it it was regarded as a cyst. It had produced

double optic neuritis and intense headache, but no other cerebral symptom whatever. I determined to expose it, and ascertain if it were removable. I found that at isolated spots, over an area of five inches, little portions of the skull were softened by sarcomatous tissue emerging from the *diploë*. I closed the wound, and five days afterwards, because the patient was anxious to return home as soon as possible, we removed the dressing, and found that healing was practically accomplished.

But lest I become tedious, I close this history of successful cases. They are sufficient to establish the proposition that the method is effective in maintaining parts aseptic if properly carried out.

I have still a word to say as to failures. The comparative frequency of these often means simply the idiosyncrasy of individuals concerned in the management of the cases, and, indeed, there are very different records in this respect throughout my Infirmary experience. I lay little stress, therefore, on their number as the test of a method of treatment, but when analysed they may yet afford information.

I have first to record four deaths among patients treated by this plan. These were cases of trephining, of nephrectomy, of double amputation, and of amputation at the elbow. The compound fracture of the skull died in six days of extensive cerebral injury, as discovered on post-mortem examination, when, also, the wound was found almost healed and perfectly aseptic. The nephrectomy has already been referred to. The amputation of thigh and leg died the same night from his internal injuries.

The amputation of the elbow is the only one which can be placed to the discredit of the treatment. It was performed in a woman, *æt.* 72, for sarcoma of the ulna. The wound immediately became septic, a considerable amount of cellulitis followed, and the patient died on the twenty-ninth day, worn out by the pain, inflammation, and discharge. The lesson to be drawn from it is one which may be enforced by two other cases in which, although recovery ensued, the local accidents were identical. One was a double amputation, tarso-metatarsal on the one side, below the knee on the other. The foot healed with one dressing, but the leg, although making an admirable stump in the end, was attacked by sloughing cellulitis. At the time of the operation I observed, too late, that a serious error had been committed by one of the assistants, which of necessity conveyed septic material to the wound. This I endeavoured to remedy afterwards by injecting it forcibly with carbolic glycerine after the sutures had been inserted. It is curious how slowly a lesson of this sort is learned. I had seen the same result in other hands with other antiseptics, and I had injected with a like effect in the amputation of the elbow for a precisely similar reason. Of course I had done it in others without mischance. The sloughing occurs if the injection be unsuccessful. The combined effect of septicity and antiseptics is too much for

any tissue. The third case was similar. A compound Pott's fracture of the fibula with opening of the ankle-joint was injected with carbolic glycerine. In this case it was not the force used, but the absence of a drainage-tube, which led to cellulitis. It has recovered well, though slowly, but the antiseptics, too long kept in contact with the texture, did harm when the part became septic. This case contrasted strongly with the other compound fracture into the ankle-joint, which I have already related, and which happened to be under treatment at the same time.

A certain number of other cases became septic, as judged by the odour, the discoloration of the protective, and other signs. These were an excision of the testicle and a hernia from displacement of dressings by the patients, one excision of the mamma, and one excision of the knee. Altogether seven cases became evidently septic,—a proportion, in my experience of Infirmary work, as small as by other methods.

If we judge by microscopic examination of the discharges, the proportion in which bacteria or micrococci could be found was very much smaller than under the gauze and Mackintosh. I do not attribute this to any more powerful germicide properties in the dressing; I believe it to be due to the fact that a far larger number of the cases are absolutely dry, and thus afford no possible soil for development. It appears to me, however, a fact of great significance, that, notwithstanding the absence of the spray, fewer cases are scientifically septic.

I append a table of all the major operations, in cases with unbroken skin, which I have personally performed in the Infirmary, and treated by this plan, since I finally abandoned the spray. I find that, including these and all others, during the past twelve months I have performed forty-one major amputations and excisions, and seventy-nine other operations of a like magnitude, on patients in the wards of the Infirmary, besides many other minor amputations and operations involving a breach of surface. Of these six have died,—

- 1 excision of the tongue—from septicæmia.
- 1 lithotomy—from erysipelas of the head, to which he was very liable.
- 1 nephrectomy.
- 1 colotomy—of diarrhœa, nine weeks afterwards.
- 1 amputation of leg and thigh—the same night.
- 1 amputation of elbow—of sloughing cellulitis.

I mention these figures because they imply a not unhealthy condition of the wards generally during the period involved in the tables. I have already said that no great reliance is to be placed on statistics in a matter of treatment; but I have endeavoured to give such particulars of the individual cases as may, to a certain extent, enable one to judge of their value, without making them unnecessarily bulky.

TABLE OF MAJOR OPERATIONS.

| No. | Name. | Nature of Operation. | Age. | Date of Operation. | Date of Dismissal. | Cured. | Died. | Remarks. |
|-----|----------|---------------------------------------|------|--------------------|--------------------|--------|-------|---|
| 1. | E. L. | Myxoma of abdominal wall. | 8 | 15th Nov. 1883. | 1st Dec. 1883. | 1 | 0 | One dressing. Tumour size of hen's egg. |
| 2. | J. S. | Cyst of thigh. | 37 | 13th Nov. 1883. | 18th Dec. 1883. | 1 | 0 | Wound healed without pus, but phlebitis of other leg delayed dismissal. A blood cyst under the hamstrings. |
| 3. | J. R. | Large cyst of scalp. | 50 | 22nd Oct. 1883. | 15th Nov. 1883. | 1 | 0 | One dressing. |
| 4. | M. M'K. | Excision of patellæ bursa. | 22 | 22nd Oct. 1883. | 15th Nov. 1883. | 1 | 0 | One dressing. Tumour not removed. |
| 5. | A. S. | Sarcoma of skull. | 26 | 29th Jan. 1884. | 5th Feb. 1884. | 1 | 0 | One dressing. Tumour on right side. Aseptic slough on left. |
| 6. | M. M. | Fatty tumour in each groin. | 47 | 12th Feb. 1884. | Still in Ward. | 1 | 0 | Six dressings. Now healed. Aseptic. Drainage difficult from decubitus. |
| 7. | M. S. | Large fatty tumour of buttock. | 30 | 21st Feb. 1884. | Still in Ward. | 1 | 0 | Two dressings. Size of goose egg. Drainage-tube of plaited catgut absorbed by second dressing. Fourteen days. |
| 8. | H. D. | Sarcoma under sterno-mastoid. | 56 | 19th Feb. 1884. | 5th March 1884. | 1 | 0 | One dressing. |
| 9. | G. H. | Lymphoma of neck. | 24 | 1st May 1883. | 19th May 1883. | 1 | 0 | One dressing. |
| 10. | J. R. | Adenoma of mamma. | 28 | 15th Jan. 1884. | 31st Jan. 1884. | 1 | 0 | One dressing. |
| 11. | A. S. | Scirrhus of mamma. | 47 | 6th April 1883. | 19th April 1883. | 1 | 0 | Two dressings. |
| 12. | M. T. | Scirrhus of mamma. | 73 | 17th May 1883. | 7th June 1883. | 1 | 0 | Aseptic. |
| 13. | E. S. | Scirrhus of mamma. | 45 | 15th May 1883. | 2nd June 1883. | 1 | 0 | Aseptic. |
| 14. | B. M. | Scirrhus of mamma. | 42 | 22nd May 1883. | 14th July 1883. | 1 | 0 | Aseptic. Extensive operation. Glands in axilla. |
| 15. | H. H. | Scirrhus of mamma. | 56 | 12th June 1883. | 18th July 1883. | 1 | 0 | Became septic, but healed well. |
| 16. | A. H. | Scirrhus of mamma. | 53 | 10th July 1883. | 1st Sept. 1883. | 1 | 0 | Became septic. Skin deficient. |
| 17. | J. D. S. | Trepthing, for compound fracture. | 66 | 5th Dec. 1883. | 10th Dec. 1883. | 0 | 1 | One dressing. Died of cerebral injuries. |
| 18. | P. H. | Trepthing, for compound fracture. | 52 | 2nd Jan. 1884. | 24th Jan. 1884. | 1 | 0 | One dressing. |
| 19. | G. B. | Resection of humerus in stump. | 17 | 17th Jan. 1884. | 29th Jan. 1884. | 1 | 0 | One dressing. |
| 20. | P. T. | Section of tibia, for deformed union. | 46 | 7th Jan. 1884. | 31st Jan. 1884. | 1 | 0 | One dressing. No pus found, but inflammation cured. |
| 21. | M. M. | Incision of tibia for ostitis. | 33 | 14th Jan. 1884. | 22nd Feb. 1884. | 1 | 0 | Two dressings. |
| 22. | J. N. | Section of tibia, for deformed union. | 36 | 28th Feb. 1884. | Still in Ward. | 1 | 0 | Not yet dressed. Is perfectly well. |
| 23. | J. T. | Section of radius, for osteoma. | 18 | 4th Jan. 1884. | 11th Feb. 1884. | 1 | 0 | Three dressings. |
| 24. | A. C. | Incision of elbow. | 18 | 19th Feb. 1884. | Still in Ward. | 1 | 0 | Has not been dressed. Temp. normal. Free from pain. |
| 25. | M. S. | Femoral hernia. | 45 | 9th May 1883. | 9th June 1883. | 1 | 0 | Aseptic. |
| 26. | M. M. | Femoral hernia. | 65 | 13th Feb. 1884. | 3rd March 1884. | 1 | 0 | Became septic from displacement of dressings, but healed well. |
| 27. | J. D. | Femoral hernia. | 57 | 6th June 1883. | 1st July 1883. | 1 | 0 | Aseptic course. |

| No. | Name | Operation | Date | Place | Result | Remarks |
|---------------------------|-------|---------------------------------|--------------------|------------------|--------|--|
| 28. | J. B. | Nephrectomy. | 31st July 1883. | Still in Ward. | 0 | Case related in paper. |
| 29. | S. R. | Excision of testicle. | 26th Feb. 1884. | | 1 | Dressings were displaced by patient, but healing nearly accomplished. |
| 30. | P. H. | Knock-knee. | 26th June 1883. | 16th Oct. 1883. | 1 | Referred to in paper. One dressing. |
| 31. | M. T. | Knock-knee. | 27th Feb. 1883. | 26th Mar. 1883. | 1 | One dressing, one side; other treated by apparatus. |
| 32. | W. M. | Knock-knee. | 16th Mar. 1883. | 16th April 1883. | 1 | One dressing, double. |
| MAJOR AMPUTATIONS. | | | | | | |
| 1. | J. H. | Thigh, for dislocation of knee. | 22nd May 1883. | 7th July 1883. | 1 | Four dressings. |
| 2. | P. M. | Thigh, for dislocation of knee. | 21st May 1883. | 5th July 1883. | 1 | Two dressings. |
| 3. | A. C. | Thigh, for gangrene. | 62 29th Nov. 1883. | 23rd Feb. 1884. | 1 | One dressing. Healed in fourteen days. Arteries were like pipe stems. |
| 4. | M. M. | Thigh, for dislocation of knee. | 17 31st May 1883. | 10th July 1883. | 1 | Aseptic. Healed in three weeks. |
| 5. | J. M. | Thigh and leg. Railway injury. | 50 13th June 1883. | 11th July 1883. | 0 | Died the same night. Shock and internal injuries. Aseptic. |
| 6. | J. S. | Leg, for ununited fracture. | 4 19th June 1883. | 9th May 1883. | 1 | One dressing. Recurrent disease after excision three years before. |
| 7. | M. S. | Ankle, for strumous disease. | 15 6th April 1883. | 11th July 1883. | 1 | Aseptic. Eight dressings. Surface slow of healing. |
| 8. | M. M. | Ankle, for strumous disease. | 32 12th June 1883. | 9th Feb. 1883. | 1 | Small portion of injured flap sloughed. Quite healed. Referred to in paper. |
| 9. | A. D. | Ankle, for gunshot wound. | 17 17th Nov. 1883. | 8th Aug. 1883. | 1 | One dressing. |
| 10. | R. W. | Leg and foot. | 42 9th Jan. 1884. | 14th Aug. 1883. | 1 | Died from diffuse ecchymosis. |
| 11. | J. J. | Arm, for burn. | 23 3rd July 1883. | 17th Nov. 1883. | 0 | One dressing. Found healed 9th Nov. |
| 12. | A. S. | Elbow, for sarcoma. | 72 17th July 1883. | 17th Nov. 1883. | 1 | Found healed on 16th Feb. First dressing. No drainage. |
| 13. | J. H. | Forearm, for injury. | 36 24th Oct. 1883. | | 1 | |
| 14. | A. D. | Forearm, for injury. | 22 6th Feb. 1883. | | 1 | |
| EXCISIONS. | | | | | | |
| 1. | M. W. | Excision of knee. | 16 31st May 1883. | 11th Sept. 1883. | 1 | Became septic. Amputation by Dr MacLaren, 11th Sept., for recurrence of bone disease, sinus never having healed. |
| 2. | J. B. | Excision of knee. | 17 24th May 1883. | 6th July 1883. | 1 | Four dressings. Pressure ulcer from splint delayed. |
| 3. | J. M. | Excision of knee. | 18 18th Dec. 1883. | Still in Ward. | 1 | Wound healed in seventeen days. Now encased in plaster. |
| 4. | G. L. | Excision of elbow. | 17 8th Jan. 1883. | 7th Feb. 1883. | 1 | Five dressings. |
| 5. | R. W. | Excision of elbow. | 9 29th Jan. 1884. | Still in Ward. | 1 | Healed by first intention. |
| 6. | W. M. | Excision of wrist. | 12 22nd Jan. 1884. | 29th Feb. 1884. | 1 | Dressed only 6th, 15th, and 29th Feb. |
| 7. | F. D. | Excision of wrist. | 15 12th Feb. 1884. | Still in Ward. | 1 | Doing well. Referred to in paper. |

There are some points which I should have liked to bring out more clearly. Thus, instead of the date of dismissal I should have preferred to give the day on which the wound was found healed. This is, of course, much earlier, as a patient is rarely sent out the moment a wound has closed. Again, I should have liked in each case to give the precise number of dressings; but these matters have not always been accurately recorded in the case books.

It will be seen, however, that of the fourteen amputations six, of the seven excisions two, and of the thirty-two other operations fifteen, were only dressed once. When two, three, or four dressings are recorded, they were performed for the removal of the drainage-tube, for early oozing, sometimes simply from curiosity. It is impossible to exaggerate the importance of this infrequent dressing as adding to the safety, comfort, and rapidity of healing.

Four deaths have occurred. One only has connexion with the mode of dressing, and I have already commented on it. The deaths from trephining, nephrectomy, and double amputation must have happened, whatever the mode of dressing. I might legitimately have added to the tables six cases of compound fracture, all of whom recovered.

I claim that these cases confirm the statements made in a former paper as deductions from experiments with the spray, and show that, from an antiseptic point of view, there is no appreciable difference whether an operation be performed with or without it; and that, from every other point of view, these alterations in wound treatment are greatly for the better.

The President asked Mr Duncan what was the strength of the preparation of carbolic acid and glycerine he used, and whether he had seen symptoms of carbolic acid poisoning from its use?

Mr Duncan replied that he used the preparation in two strengths, 1 to 20, and 1 to 12: 1 to 4 might, however, be used without causing irritation. He had not observed symptoms of poisoning.

Mr Joseph Bell said that Mr Duncan's papers for the last three years had been gradually converting him to his (Mr Duncan's) views. He had now almost altogether given up the spray. There were a few special cases in which, possibly from some superstitious notions, he retained it still. The bichloride solution, he thought, caused less serous discharge than the preparation of carbolic acid and glycerine, but his experience of either had not been very considerable as yet. Their results without the spray for the last few months had certainly not been worse than before. There had been no preventible mortality. As the Society was aware, he published his hospital results every six months. It might be interesting to compare those in which the spray had played no part with those previously published. In the course of another twelvemonth there would be very few spray-engines left.

Prof. Annandale said he should like that the Society and the pro-

fession thank Mr Duncan for the work he was doing in connexion with antiseptic surgery. There were few surgeons who did not practise antiseptic surgery in some form or other. Great credit was due to Mr Duncan for his endeavours to simplify that mode of treatment. Mr Duncan as well as himself were at one time thoroughly Listerian as regards the spray, and they always had the most thorough faith in the principles brought forward by Mr Lister and in his practice. At the same time he (Mr Annandale), speaking for himself, had the feeling that the great disadvantage lay in the complication of the spray. It was always getting out of order. That did not matter so much in hospitals, but in private practice it was a great inconvenience. Obtaining so many good results from the antiseptic treatment, he always had the hope that the spray could be done away with, and it was in great part due to Mr Duncan's work that he was encouraged to give antiseptic surgery without the spray a trial. As a result he had come to the conclusion that the spray was not necessary. During the last nine months, in hospital and private practice, he had not used it. He had carried out very much the same practice as Mr Duncan, and it had been not only successful, but a very great improvement and simplification. The cases under his care, as those who knew him must be aware, were not few. The result of his practice in connexion with operations and accidental wounds was that he was more and more convinced that the spray was not necessary. He could refer to five or six cases in which he had cut into healthy knee-joints without the spray in this fashion, and without any bad results following. He was convinced that the results he had obtained by this more simple method were not at all inferior to those attained by means of the spray. At the same time he should like to guard himself by remarking that in other respects he still carried out the Listerian principles and practice.

Dr R. Maclaren said that Mr Duncan's communication was to some extent a surprise to him. It was not altogether so, as he read with great interest the paper of last year. It was a surprise to him, because some time ago, along with many other surgeons, he discontinued the use of the spray during operation, syringed wounds with carbolic lotion, and dressed with gauze under spray. His results were unfortunate. He thought that some corner was liable to escape washing and become a source of septic infection to the rest of the wound. He felt that every surgeon would welcome a simpler method than Lister's, if it were for nothing else than the comfort of his hands.

Dr F. M. Caird said the subject was full of interest to all of them. The most important point in Mr Duncan's paper was the successful treatment of his cases of empyema and psoas abscess, which were admirable test cases for the method of treatment he had introduced to their notice. At the same time, one could hardly accept his statement that the spray was inefficient or useless,

and Mr Duncan himself had put on record a case where an empyema wound had gone wrong through some defect which occurred in the spray at one of the dressings. Now, it was extraordinary that on this one exposure when the spray went wrong the empyema should have become septic, and yet it remained all right on the previous occasions when no such defect occurred. There were cases on record where psoas abscesses had pursued a satisfactory course for many months with weekly dressings under the spray, but where momentary exposure without the spray had been followed by disastrous consequences. The dry dressing was a very important factor in the success attending Mr Duncan's treatment. He doubted if they would get good results from the glycerine preparation used along with the gauze and Mackintosh. As to the experiments made, if the spray were inefficient they would not get good results from inoculating flasks under the spray. The results of such inoculations had varied with different individuals, with draughts, and with the character of the air in the room, and, lastly, the germs found had not been of necessity septic germs. Micrococci and bacilli had certainly been found, but they did not, of necessity, cause putrefaction. Only in certain places and under certain conditions did Klein's test tubes swarm with organisms when exposed under the spray, and then it was some special form of bacillus that was present. In the same way he had himself seen a peculiar form of organism in a series of wounds which were pursuing a normal aseptic course, in all of which the use of an inferior sample of catgut had caused the inoculation.

Dr Heron Watson said there was a certain satisfaction in patiently waiting upon the retributive justice meted out by Time the avenger; especially so when one came to find that the views, which one held at a time when it was strongly hinted that those who held them were weak and foolish, obstinate and antiquated, were now being gradually accepted by the very persons who previously decried them. One after another of the various essential parts of the so-called "antiseptic system" had been given up as unnecessary, until pretty nearly the whole edifice had crumbled down, leaving very little of it standing. They had just heard to-night that bacilli were nothing, that micrococci were nothing, that bacteria were nothing, but that decomposition was everything. The whole question was begged in that statement. What had the bacillus, what had the micrococcus, what had the bacterium to do with the putrescent and with the suppurative changes in the wound? Any one who refused to accept all that Sir Oracle said a very short time ago regarding these matters was supposed to be incapable of practising surgery, and utterly unfit to be entrusted with the teaching of the rising generation of surgical practitioners. Such statements had been made, and not only made, but used to injure certain individuals. But it was well to wait, for when they waited they found that those very people who

made such statements now turned round and said that those articles to which they formerly professed to have pinned their faith were not of importance, and that, in fact, they might be abandoned altogether. There was no surgeon within the last quarter of the century who did not believe in antiseptic principles, it was the methods alone which, constantly undergoing transformation, were unreliable. No doubt novelties were interesting, and new proceedings were attractive. Elaborate instruments, with mysterious joints and screws and sprays, whether worked by hand or by engine, were matters of the deepest interest to ingenuous youth, and very much calculated to draw together an admiring crowd of students, sometimes even to entertain them with the possible dangers of an explosion. After all, the whole thing was now ending very much in the fashion of the usual close of a pantomime. But when one came to consider what was the efficacy of the various methods of carrying out antiseptic principles, one could not help feeling that one grain of truth was infinitely superior to all the tons of theory which had been brought to bear on the matter from every possible quarter. It positively seemed, in the opinion of some authorities, that everything which succeeded was claimed as antiseptic surgery, everything which failed sinned against the principles of that mysterious faith. He much admired Mr Duncan's paper from the manner in which facts predominated. The results were in harmony with his own experience. He hoped the time would arrive when the young gentlemen who had been so thoroughly educated in those passing fashions would be able, as they gained some personal experience, to eliminate from their practice those mysterious methods which probably during their student years they were hardly capable of properly appreciating.

The President said he was not surprised at some of the remarks made by Dr Watson. He was very much interested in Mr Duncan's statements, more particularly as about two or three years ago he was instructed by the managers of the Infirmary to communicate with the Water Company, to ask them to turn on an extra quantity of water to work a spray in the large theatre. The gentleman who had made this request of the managers was Mr Duncan, and he could hardly believe his eyes when he saw him coming forward that night to say that the spray was of no use.

Mr Duncan replied that there was no reason why he should not think one thing at one time and another at another. Every one who had a mind of his own must do so. A few years ago he applied to the managers to have a spray fitted up in the large theatre of the Infirmary. The managers, who at that time were somewhat penurious, refused to supply steam-sprays, which were a serious item of expense to the surgeons. He suggested to them the propriety of putting a spray in the large theatre, and added, as a matter of economy, that they might get the Water Company to supply water to work it instead of steam. That was

the history of the transaction to which the President referred. He believed then in the spray, and that it was of the utmost importance for the patient to keep the causes of putrefaction from entering the wound, and he regarded the spray as one of the means whereby they might be kept out. He was prepared to accept this as a fact, because of the statements made by others, and because he saw himself that it did no harm to the wound. He believed, therefore, that not doing harm it was probably an essential part of the antiseptic system. It was true that he had similarly changed his mind regarding the case of empyema. The case was one in which, for several dressings, the discharge had remained aseptic. During a prolonged exposure the spray went wrong. Two days after the temperature rose, the pus increased, organisms were found in it, and it had an odour. The change was distinct. He then attributed it to the spray going wrong, but this went wrong only for a moment. The wound was at once covered up. He believed now, after having made his experiments, that he was wrong in attributing the change to the defect in the spray. He believed now it was due rather to the lengthened exposure which he allowed, because of his confidence in the spray. During this exposure air entered the pleura with every expiration, and there was no doubt the spray was unable to destroy organisms which might have entered. On the other hand, a wound might be exposed for some time without becoming septic. It took a considerable time for a surface of three inches to become contaminated with certainty. A beaker of three inches took twenty minutes to become certainly septic. In certain places it might become septic in one minute. Once he had one exposed for fifteen minutes in his room in the Infirmary, then covered with a septic material, and it remained quite pure. The germs were also capable of being destroyed by the tissues. They must go into a wound in numbers to do harm. They must have gone in along with the spray in his case of empyema. The former process of dressing was not so effective in destroying or neutralizing them as his present mode, because the spray did not kill in time. Most recent experiments showed that microbia must be exposed to the action of carbolic acid for some time before being rendered innocuous. A germ entering with the spray might be dazed and stupid for a little, but it comes to again, and might do harm. A very interesting point with reference to antiseptic vapours was, that in order to make the atmosphere aseptic, they must charge it for a considerable time. For example, in experimenting with eucalyptus, they laid down a certain quantity and made it throw off its vapour for an hour. They then put in a beaker, exposed it for an hour. It became septic. At the next experiment they threw off the vapour for two hours instead of one, and they kept the beaker in for five hours without its becoming septic. It might be said this was due to subsidence; it was

not, because he tried again without the eucalyptus, and let it subside, and the beaker became septic. The conclusion he had come to from all these experiments was that the spray was at the best useless, and that it might even do harm, because at its margin it was drawing in a quantity of septic air. The margin of the spray was the most dangerous part in the room. It could not be said that any germ which might be whisked in would be killed by the spray.

Meeting VI.—April 2, 1884.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

I. ELECTION OF HONORARY MEMBERS.

Sir William Jenner, Bart., London, and Professor Brown Sequard, Paris, were elected Honorary Members.

II. ELECTION OF CORRESPONDING MEMBERS.

Professor Pasteur, Professor Pettenkoffer, Professor Ollier, Professor Ask, and Dr Fordyce Barker, were elected Foreign Corresponding Members.

Sir J. Lister, Bart., Sir J. Fayrer, K.C.S.I., Dr J. Matthews Duncan, Dr J. Syer Bristowe, John Eric Erichsen, F.R.C.S., John Marshall, F.R.C.S., London, Professor Struthers, Aberdeen, and Professor Gairdner, Glasgow, were elected Corresponding Members within the United Kingdom.

III. ELECTION OF ORDINARY MEMBERS.

Alex. Thom, jun., M.D., C.M. Ed., M.A. St And., Crieff, and Hugh L. Calder, M.B. and C.M., Leith, were elected Ordinary Members of the Society.

IV. EXHIBITION OF PATIENT.

Dr W. A. Finlay showed a patient who was the subject of AMPUTATION AT THE HIP-JOINT FOR ACUTE DISEASE. The operation was performed, after consultation with Dr Watson, eight months ago, in the following manner:—An incision was made down to the bone, commencing about an inch above the acetabulum and extending along the neck of the femur and the centre of the great trochanter, to a point below its inferior border. The soft parts were then dissected from the bone, and the head of the bone was removed from the acetabulum. At this point it became evident that the disease involved, as was anticipated, so much of the shaft of the femur as to render amputation necessary. The incision was therefore extended down the outer side of the femur as far as seemed necessary to obtain sufficient length of flaps. At

this stage of the operation an elastic band was applied firmly round the soft parts, between the pelvis and the dislocated femur. The flaps were then completed by transfixion, the limb was removed, and the vessels tied with scarcely any loss of blood. Five months later, viz., at the end of last December, a large piece of necrosed bone was removed from the acetabulum, and a considerable quantity of carious bone gouged away from the ischium. The cicatrix of the incision necessary for this operation, which was somewhat tedious, was seen in the posterior flap over the acetabulum. There was still a very slight dampness on the dressing, but no sign of there being any further disease of the bone. The patient was one of a strumous family and small of his age, which was stated to be 16 years. He was admitted to Leith Hospital on June 8th. Up till three days before admission he had been quite well, but on the morning of June 5th he felt a slight pain in the right inguinal region. This pain increased in severity during the day and caused him to leave his work, to which he had gone in the morning. On admission there was found to be some swelling at the seat of the pain, which was extreme. The swelling gradually increased and, on the 10th of July, 20 ounces of pus were evacuated by an incision in front of the thigh, where deep-seated fluctuation was well marked. On July 22nd the limb was amputated at the hip-joint as already described. Thus only forty-seven days had elapsed from the time he first felt the pain until the date of the amputation. With reference to the means employed to command the hæmorrhage, Dr Finlay could not speak too highly of the efficacy and simplicity of the elastic tourniquêt used in the manner suggested to him by Dr Watson.

V. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Dr W. A. Finlay* showed the HEAD AND NECK OF THE FEMUR from his case of amputation for hip-disease. In the space of forty-seven days the disease had completely divided the head of the bone, as was seen in the specimen.

2. *Dr C. W. MacGillivray* showed ONE-HALF OF THE LOWER JAW REMOVED FOR MALIGNANT TUMOUR. The preparation was interesting chiefly because of the history of the case. About a year before, a small epithelioma of the lip had been removed by another surgeon in the hospital. Nine months later a tumour appeared on the side of the jaw. It appeared to be a sarcoma of the jaw; but the history made the diagnosis somewhat more difficult. The operation showed that the first surmise was correct. The patient had gone on well.

3. *Prof. Chicne* showed (1) A CALCULUS from a boy, aged 5 years, who had been sent by Dr Hutchinson of Cockermonth, with the history that he had very frequently to make water during the day and night. On Nov. 14 of last year, under chloroform, a small

sound was with difficulty passed into the bladder. A grating sensation was felt as it passed through the membranous urethra. No stone could be detected in the bladder. But by the rectum a stone was felt lying in the membranous urethra. A staff was then passed as far as the calculus, an incision was made in the raphe of the perineum, and membranous urethra opened into. An attempt was then made, with the finger in the rectum, to push the stone out of the wound, but it now slipped back into the bladder. An incision was then made through the prostate, and the stone was removed. The thicker end of the stone was in the bladder, the other end forming a mould of the membranous portion of the urethra. (2) AN OVARY which he had removed from a child aged 3 months, who had been sent into hospital by Dr Spence of Burtisland. A swelling appeared quite suddenly in the inguinal region, the mother says, on the night of the 7th February. It was not noticed at 7 P.M., when child was put to sleep, but at 10 P.M., when child's clothes were being changed, she noticed the swelling. Dr Spence applied hot cloths, and under chloroform attempted to reduce the swelling, but did not succeed. Three days after swelling appeared child was brought to the hospital. The mother says the swelling was no larger then than when it first appeared. Its bowels have been quite regular all the time. On the 11th and 12th of February, hot fomentations applied by Mr Chiene's directions. On the 13th the child was put under chloroform. An incision was made over the swelling, and the sac dissected down to and opened. A ligature was placed round the proximal end of contents, and the neck of the contents divided, and the contents removed. The mass excised was found to be an ovary with Fallopian tube. The wound healed by first intention, and patient left on the 19th of February.

4. *The President* showed THE PARTS INJURED IN A CASE OF CUT THROAT.—The case was interesting in its medico-legal relations. The larynx was extensively mutilated. The thyroid cartilage, which was implicated, was broken up, being divided transversely, and the left superior cornu was detached. The lower third of the cartilage together with the crico-thyroid membrane was wanting. The wound in the skin, which measured 5 inches across, passed obliquely upwards on the left side, and at each end of the wound there were several slight incisions, showing that several attempts had been made. On the right side the sterno-mastoid muscle was almost completely divided, but neither the artery vein nor *descendens noni* nerve were injured. On the left side the sterno-mastoid was completely divided. The common carotid was exposed below its division but not injured, while the internal jugular vein was cut transversely but not completely divided. The *descendens noni* nerve was exposed but not injured. The patient was a street porter, æt. 64, who for some days was strange in his manner and

had excited the suspicions of his friends. He left his home after breakfast apparently well. He was found accidentally by a message boy, at seven in the evening, in an area of a dwelling-house half a mile distant from his home. He was in a half-sitting position and quite dead. A large knife used for cutting ham was found smeared with blood about seven yards from the body. This knife belonged to himself, and he had been observed narrowly examining it. What took him to the locality was never ascertained. Through the kindness of Dr Keiller, the parts in a similar case of extensive laryngeal mutilation were shown. The case was reported to the Society and published in the *Edinburgh Medical Journal* for December 1855. Both these cases were suicidal, as was proved by various circumstances; but the peculiar mutilation, which was rare, pointed either to suicide or the effects of delirium arising from drink or insanity. It was interesting to note that in Dr Keiller's case the patient was a victim to delirium tremens, while in the present there could be little doubt that insanity was the cause of the suicide.

Mr Joseph Bell reminded the President that he had once been associated with him in a very remarkable case of suicidal mutilation of the larynx, in some respects not unlike the one now shown. Had the man not been actually seen to cut his throat, it would have been exceedingly difficult to believe that he had done it himself. He was a patient in one of the wards of the Infirmary. Two other patients saw him thrust a small blunt pocket knife, used for cutting tobacco, into his throat and actually cut or haggled out from behind. He wounded a large vessel. One of the patients stopped the bleeding by putting his finger on the part. Mr Bell had to tie the carotid; but the patient died eventually of intra-lobular suppurative pneumonia.

VI. EXHIBITION OF OTHER SPECIMENS.

1. *Prof. Chiene* showed a PHOTOGRAPH OF A CHAIR, which, along with an escritoire, had been presented to the University of Edinburgh to be kept in the surgical department. These articles belonged to the late John Hunter, and had been presented by the representatives of the late Professor Goodsir. The chair was one of twelve made of the wood of the *Acacia melanoxylon* which had been brought home by Joseph (afterwards Sir Joseph) Banks when he was botanist in Captain Cook's expedition. The wood was presented to John Hunter, who got twelve drawing-room chairs made of it. Sometime after Hunter's death the chairs were found in a lumber room below the College of Surgeons in London. The late Mr Stone got the desk and advised Mr Goodsir of the sale of the chairs. Mr Goodsir commissioned him to purchase three, and offered him double of what he paid for the writing-desk, which offer was accepted. The late Mr Frank Buckland had a chair

made out of the wood of Hunter's bed, which was now in the possession of Mrs Buckland. After her death, he believed, it was to go to St George's Hospital.

2. *Dr Church* showed a *STUD*, about the size of a shilling, which had been swallowed by a child two years of age. The parents were about to administer purgative medicine; but he had dissuaded, and the stud passed naturally and without injury to the child in two days.

3. *Dr Foulis* gave a demonstration of the circulation in the gills of the tadpole.

VII. ORIGINAL COMMUNICATION.

CHRONIC POISONING BY BISULPHIDE OF CARBON.

By ALEXANDER BRUCE, M.A., M.B., F.R.C.P.Ed., Lecturer on Pathology, School of Medicine, Edinburgh.

THE following three cases of this form of poisoning came under my observation at the Western Dispensary, Fountainbridge, and seem worthy of the attention of the Society, as they are apparently the first described in this country. The subjects of it worked in the "curing-house" in one of the rubber-works here, their duties requiring them to dissolve chloride of sulphur in bisulphide of carbon, and to superintend the working of the machine for vulcanizing the cloth. They are thus almost continuously exposed to a more or less concentrated atmosphere of the bisulphide.

CASE I.—J. M., a healthy looking man of 49 years of age, with an unexceptionable family and social history, was the first to work in the "curing-house." For six years he had been employed in the same factory as a "spreader" of the rubber dissolved in naphtha on the cloth. Though much exposed to the naphtha vapour, he never suffered in the very least from it. About September 1880 he was transferred to the "curing-house," where for about two years, owing apparently to his not having been very closely confined to the work, he does not seem to have suffered very much beyond occasionally vomiting his food—an act which was always followed by immediate relief from the nausea produced by the inhalation of the vapour. About October or November 1882, a change in the mode of manufacture led to greater concentration of the vapour; and J. M., who was now constantly exposed to its influence, immediately began to suffer. He lost first appetite for his evening meal, became more frequently sick, vomiting always relieving the nausea as before. (J. M., who is a perfectly temperate man, tells me that he sometimes took a glass of whisky to induce emesis, which it did at once.) He found himself unable to retain his urine for above an hour, and states that the act of mic-

turition was often accompanied by a feeling of scalding, and followed by a discharge which stained his linen. He became easily fatigued, feeling constantly, on the slightest exertion, as if he had walked many miles. On this account, and partly because of a severe pain in the loins, induced by the sitting posture, he generally went to bed on returning from work. He became listless; talking and being talked to were alike a trouble to him. Then he seems to have been at times nervous and easily agitated, at others half stupid, almost entirely losing his memory, and imagining that he was going to lose his reason. At night he slept little: he had horrible dreams of falling over precipices, being hanged, committing murder, etc., and would frighten his wife by shouting aloud, starting up with the terror, and throwing his arms about wildly. On no occasion were his dreams of a pleasant nature. He rapidly lost all sexual desire and power. In December he broke down, and was confined to bed for six weeks, with great weakness in the limbs, a feeling as if the left leg were paralyzed, numbness and coldness of the feet and legs (which had lasted a long time), cramps in both calves, and dilatation of the veins of the right leg. During these six weeks the appetite was very good. At the end of this time, though not fully recovered, he returned to work, the feeling of weakness and numbness in the limbs still persisting to some extent. All the symptoms got gradually worse again, but he continued at work till about the middle of April, when he began to observe that his vision was not quite so clear as formerly. About the end of the month, after a more than usually prolonged exposure to the bisulphide, he became very sick; was seized with a "cold trembling all over." To throw off the feeling of nausea, he went into the open air, and was surprised to find, after an attack of vomiting, that he had lost the power of distinguishing objects, and that everything appeared of a bluish green hue. He says, "Everything looked as if seen through a greenish veil. I could not see the telegraph posts on the opposite side of the road, nor the clock in the steeple of the Dalry church, nor some cows 30 yards off; and yet I had plenty of light, but could only distinguish large objects quite near me." There was no night blindness. This curious condition persisted, and he says that he could not recognise his own son in the middle of the street; that he one day lost two of his sons who had strayed a little way from him on the road in the Queen's Park. Perception of small objects was so enfeebled that he could not distinguish even the type on the page of a newspaper from the unprinted margin; both were alike of the same greenish tint. He tried glasses on his own account, but found none to improve vision. He states that perception of moderately near objects was improved by means of an opera-glass, but that he could not by its aid see Inchkeith from Leith Pier. He managed to continue at his work somehow for about another fortnight, when he went to the Eye Infirmary in Cambridge Street, where he says

it was found that he could only read the largest of Snellen's types on approaching it very closely. Vision was not improved by either convex or concave lenses. Atropine was dropped into each eye, and an ophthalmoscopic examination made; but what was found, it is now impossible to say. A curious condition of vision followed: for a day afterwards everything appeared red, or rather rose coloured, instead of green. (His other special senses do not seem to have suffered in any way. He complains of having had a constant cold band extending round his forehead back to the occipital protuberance. Unlike the other cases, he does not seem to have lost flesh to any remarkable extent.) He went to the country for eight weeks, where he gradually improved; the feeling of weakness passed off from his limbs. In seven weeks the green vision had gone; he began first to see objects at a distance, and after three or four months he could see to read with glasses (of 2.5 D) perfectly. On examination (2nd April) the pupils are equal, of moderate size, react normally. There is 1 D of hypermetropia; the fundus oculi is quite healthy, and shows no trace of any previous disease. There is no colour-blindness. His other symptoms also gradually left him, and he is now, intellectually and physically, "as good a man as ever he was," as he says.

CASE II.—G. P., æt. 49, married, of temperate habits, and previously always fairly healthy; has been employed for a year in the same factory, at first as a general workman, and later regularly in the "curing-house," for about ten hours daily. For two months he did not feel any marked symptoms, except that occasionally his eyes would water profusely, and that he would feel dizzy, especially late in the day, and when walking home from his work. After this period he became rapidly worse, and began to feel that he was really ill. He lost appetite, and had a constant feeling of nausea, and after a short time vomited the greater part of his meals. He felt always tired, and on returning from work generally went straight to bed, or remained sitting in a listless condition, not caring to speak or be spoken to. He was not breathless on exertion; had no palpitation; no cough except when exposed to very concentrated fumes of the vapour in the workroom. He became unable to retain his urine for any length of time, and during the last two weeks of his work it was passed with some pain and difficulty. He became pale and emaciated. His hands, forearms, feet, and legs felt cold and benumbed, or tingling. There were frequent and severe cramps in the legs. He soon became so feeble that he could not do the work of a small boy, and that he could only walk on a level floor, and that he had to be assisted up and down stairs to and from his house. He seems to have got rapidly worse during the last three weeks of his work in the factory. He found himself sometimes, on coming home in the evenings, excited as if with alcohol, but never so bad as to be

unaware of what he was doing. Generally, however, instead of being excited he was dizzy and stupid. He suffered from intense headaches, reaching from the root of the nose round the head to the occipital protuberance. His sleep became unrefreshing, and was broken by dreadful dreams, of falling down precipices, etc., from which he would awake with a start and find himself bathed in perspiration. His memory became greatly impaired, and during the last fortnight of his work he was often at a loss for words to express himself. His special senses did not suffer in any way. After continuing at his work, and becoming daily weaker for three weeks after the first symptom manifested itself, he was removed to another part of the work, in the hope that a healthier atmosphere would restore him. His change did him no good, and he was dismissed as being of no further use on the works. So weak was he that he had to rest several times on his way home, and had to climb up stairs on his hands and knees to his house, and across the floor into bed, where he lay exhausted for some hours.

When I saw him first he was sitting up in bed with a wearied, listless expression, with his head bent forward on his chest. He replied to questions with the air of one extremely exhausted from over-fatigue. On general examination, there was nothing marked on the skin, but there was almost complete absence of subcutaneous fat, and great diminution in the size of the muscles.

In the *alimentary system*, there is still great anorexia, but now nothing else worth noting. He states that previously all his food tasted of the bisulphide of carbon.

The *circulatory system* shows nothing abnormal except a pulse of 92 per minute, a rate which increases to 110 on assuming the erect position. It is regular, and of fair volume. On examination of the blood the hæmocytes and hæmoglobin are not diminished.

The *respiratory system* is found, on physical examination, to reveal nothing worth noting. The breathing is like that of an over-tired man.

Genito-urinary System.—The urine is of specific gravity of 1020, acid, free from deposit, albumen, or sugar, and passed without scalding.

All sexual desire and power vanished after he had been at work in the curing-house for a week or two. (There was no previous increase.) The testicles are somewhat atrophied.

Integumentary System.—Absence of subcutaneous fat, and great increase of hair on forearms.

Nervous System.—Great pain and pricking in the hands are induced by washing them under a tap with cold water. Sensibility to tactile, thermal, and painful stimuli were found unimpaired. The legs are cold to the touch from the knee downwards.

On ophthalmoscopic examination, there is no evidence of present or previous disease. The pupils are of medium size, and contract

normally; convergence good; no strabismus. The other special senses are in no way impaired.

Motor Functions.—Organic reflexes normal. Skin, patellar, and other tendon reflexes could not be elicited.

His muscular system was so enfeebled that he could not rise from or sit down upon a chair without the aid of his upper extremities. When placed prone on the floor he can only, and with difficulty, raise himself on to his knees, and is quite unable to get upright on his legs without assistance. In walking, the steps are extremely short, the sole of the advancing foot is brought flat to the ground, while the knee, after giving way slightly, is extended with a jerk before the next step is taken. Turning is impossible unless he is supported by some object. The flexors of the fingers are so feeble that the act of shaking hands is impossible, and the index of a dynamometer cannot be moved from zero with the right hand, and only to 5 lbs. with the left. The flexors of the forearm are very weak, especially those of the right side. The other muscles of the arm are so much affected that he cannot place one of his hands behind his head without assistance, nor can he shave himself. When he attempts to raise a tumbler of water to his mouth he can only clutch it with great effort, and he has to hesitate for some seconds before beginning to raise the vessel. During the act there is much fibrillary tremor in the muscles of the forearm, but the vessel is carried up fairly steadily, and there is no shaking of the head. The hand becomes very unsteady in its descent.

Co-ordination is not much, if at all, impaired. Electrical examination by the faradic current showed diminished reaction to stimulus of both nerve and muscle. When the electrode was applied to a muscle only those fibres immediately underlying it contracted.

The muscular system was throughout very reduced in size, but there was no special atrophy of any group of muscles.

The treatment adopted was the internal use of *nux vomica* and faradization of the muscles, which was most regularly carried out by Mr Mallam. This has been followed by a gradual improvement in all respects, although that has been retarded by insufficient food.

CASE III. was slighter in nature, and may be briefly summarized. M. A., æt. 39, married, temperate, began work in the "curing-house" in July of last year, and broke down in November. For a month he felt no bad effects. After that he began to suffer from gaseous (CS_2) eructations, vomiting, inability to retain his water for any length of time, severe frontal headache, dizziness, coldness and numbness of the extremities, feeling of great lassitude. On leaving his work he frequently felt, and often staggered on his way home, as if intoxicated. Sleep became restless, disturbed by horrible

dreams, similar to those of the other two patients. His memory became somewhat impaired. For a time he took open-air exercise, in the hope of counteracting the evil effects of the exposure to the vapour, but was at length compelled to desist, from his increasing feebleness, and by severe pain over the base of the sacrum. He had also severe cramps in the legs during the night. There was complete impotence and loss of sexual desire. The special senses were not affected; and on examination the various forms of sensibility were not impaired. There was great diminution of the subcutaneous fat, and some decrease in size of the muscles, but no special atrophy. The galvanic and faradic reactions were diminished in amount, but there was no departure from the usual formula.

Compared with the other two, this case was very slight, and improvement under nux vomica and faradization was comparatively rapid. The faradization was at first applied to the upper extremities, which, within three weeks, regained almost their normal power. The increase in strength of grasp immediately after the application of the current was most marked, but this was only partially maintained till the next application. Similar treatment to the lower extremities was found to be similarly beneficial.

The President said he was sure the Society was much pleased with the manner in which Dr Bruce had narrated those interesting cases. When the paper was published they would be able to study the peculiar symptoms more carefully and as they deserved. From foreign sources he was aware of the effects of bisulphide of carbon poisoning; but up to the present they were unknown in English works. It was very interesting to find these cases turning up in Edinburgh, and being described by a competent observer. It was somewhat remarkable that while the use of bisulphide of carbon in rubber works in Edinburgh had not been made the subject of public complaint, that of the spirit of petroleum, which had been substituted for it, had, and it had been his duty to communicate with the directors of this work that they might take steps to prevent such escape, which was becoming a great nuisance, though perhaps not directly injurious to health. The present cases had evidently been most carefully observed. It would have been interesting to have noted the effects of the poison on the female constitution, but, he supposed, women were not engaged in this work and therefore not exposed to it.

Dr Craig was sorry he could not add very much of importance to the discussion which was so ably introduced by the President. Dr Bruce's cases were extremely interesting and exceedingly rare, at all events, to such an extent as these men had suffered. He had not himself met with any instances of this poisoning, though he had attended not a few of those engaged in these works. No

cases had come under his notice that he could trace to such a source. He should like to ask Dr Bruce if he knew how long the bisulphide of carbon had been used, also, if any, and what proportion of the workers showed an immunity to the effects of the vapour. The treatment that had been adopted was very successful. He thought that if he got a case of the kind he should like to try phosphorus, which was a strong nervine tonic. It was interesting to note that in America the bisulphide of carbon was used as an external application in such cases as deafness from a deficiency of wax or nervous affections, in tic and other neuralgias, and was said to be very successful. It was one of the officinal remedies of the U.S. Pharmacopœia.

Mr Joseph Bell said the cases reminded one of the curious symptoms observed in post-diphtheritic paralysis. He believed that the pathology of this malady was now held to be a paralytic condition commencing in the vasomotor system. Might not this poison cause a similar vasomotor disturbance which would lead to the symptoms described?

Dr P. A. Young was struck with one of the symptoms mentioned by Dr Bruce. It was the feeling, complained of by all three men, of a tightness or band round the head. Such "iron-band" symptom was familiar to them as occurring in a variety of diseases. A case in which this symptom was very marked came under his notice a good many years ago. The patient, a man, suffered great agony, and when first seen by Dr Young was, on account of the severity of the pain, dashing his head against the bed. He was not quite sure as to the cause, and was at his wit's end to know what to do with him, as all ordinary remedies failed to give relief. It so happened that he had been reading in an American paper of the use of bisulphide of carbon in neuralgias and headaches. He accordingly applied it as recommended. Putting ten drops on a little wadding in a wine-glass he held it to the man's forehead, where the pain was most intense, till the leech-bite sensation was produced. The pain then ceased and was only felt behind, where another application of the remedy also relieved it. It recurred but once, and was speedily overcome in the same way.

Dr Argyll Robertson said that in listening to this able and instructive paper he was particularly struck by one symptom complained of by one of the patients, a very peculiar visual phenomenon—everything appearing of a greenish or bluish-green colour, and after the application of a drop of atropine a change, lasting for twenty-four hours, in which everything appeared of a reddish tint. It was difficult to give an explanation of these phenomena. Coloured vision was very rare in eye affections. In addition there was the extraordinary dimness which a simple defect in the refracting media would not account for. They sometimes had patients complaining of coloured vision when blood was

extravasated into the anterior chamber; but in such cases the ophthalmoscopic examination very soon revealed the nature of the lesion. Besides, the colour in this instance was not typically that due to the presence of blood or blood-colouring matter in the media of the eye. They must therefore exclude that. They did occasionally find other cases of coloured vision. One was the familiar poisoning of the nervous system by santonin. Another form of coloured vision of considerable interest might result from accidents in which the nervous system has got a severe shock. He remembered well reading an account of a medico-legal case, described, he thought, by Wharton Jones, in which a man got considerable damages from a railway company, on account, not of impaired sight, but of coloured vision, occurring after a railway accident. His occupation having been that of a grain-dealer, it was of importance to him to have a good clear vision; but after the accident everything had a yellowish tinge, so that he was unable to distinguish the various kinds of grain. Except these three different causes, so far as he was aware, he was not acquainted with any other that gave rise to coloured vision. It was very interesting to note that in this case it seemed due to poisoning of the nervous system by the vapour of bisulphide of carbon. He did not know if it had occurred in other cases, but it would be interesting to observe whether the same symptoms appeared or not. It was curious that in one of the cases, in which the nerve symptoms were greater, this coloured vision did not exist; but it was possible that different parts of the nervous system might be affected in certain individuals, and that in this instance the optic nerves chiefly suffered.

Dr James asked *Dr Bruce* if he had any explanation to give regarding the curious effect of the atropine. The bisulphide of carbon appeared to have caused the greenish hue; the atropine, on the contrary, brought out the colour which was the complementary of green, namely, the reddish tint. It appeared as if the atropine had some opposite effect to that of bisulphide of carbon, but it was a matter on which a good deal might be theorized.

Dr Hare asked what was the pathology of this condition, was it purely functional or had it any organic substratum? He was inclined to take the former view, and was rather of opinion that the symptoms might be explained as due to a constant hyperstimulation of peripheral end-organs, more particularly the olfactory. He would like to know if *Dr Bruce* had thought of such an explanation.

Dr Bruce, in reply, said that the French physician, who first described these cases as they had occurred in Paris, recommended the internal administration of phosphorus along with faradisation. He had, and he thought wisely, kept clear of theories regarding pathology, because, so far as he knew, no case of chronic poisoning

had proved fatal. He had theorized a good deal about it, but Dr Hare's explanation had not struck him. It seemed to him that there was a saturation of the whole system, seeing that the bisulphide was excreted by the skin, lungs, and kidneys. Bisulphide of carbon was a solvent of fats, and apart from the emaciation produced by loss of appetite and vomiting, it might be that the fat was actually dissolved out of the body, but he had no means of proving that. With reference to the tight feeling round the head, he thought it might partly be due to the vapour getting into the frontal sinuses; but that also was a thing which he could not prove. He agreed with Dr Robertson that the dimness and coloured vision were due to poisoning of the nervous system. He had formed no theory regarding the very remarkable effect of atropine. This case was the first, so far as he knew, in which the green vision had been noticed. Others had suffered from defective sight. With regard to other questions, the bisulphide had been used for four years in the works. The first patient was the first man to work in it. During the first two and a half years he did not seem to have been constantly in the room. Some other men who were beginning to be affected took fright and left. These three were the only severe cases; but every other man who worked in the room suffered to a slight extent. They worked for a month or two, then the vomiting and other symptoms of prostration began to show.

Meeting VII.—May 7, 1884.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

I. ELECTION OF ORDINARY MEMBER.

J. Craig Balfour, L.R.C.P. and S. Ed., Edinburgh, was elected an ordinary member of the Society.

II. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Dr Wm. Russell* showed (*a.*) A LUNG FROM A CASE OF TUBERCULOSIS, which he had removed from a child 4 years and 9 months old, who had been under his care for fully two months. The case, although not uncommon, presented some points of clinical interest. The general condition during the early period of his attendance was one of debility and listlessness with slight cough. The temperature was febrile. There were no perspirations and no diarrhœa. The tongue was clean and moist. There was a family history of phthisis. At the extreme base of the left lung posteriorly there was a localized area of dulness, over which coarse râles were heard. The physical signs over the rest of the

lungs were normal. The condition then appeared to be such as is frequently seen in children making a bad recovery from pneumonia. Treatment was of no practical benefit; the child did not improve. Emaciation progressed; the temperature attained a higher range, reaching 103° or more at night, and falling to about 100° in the morning. There was a total absence of perspirations; the cough was a little more marked, and the respirations became somewhat more accelerated. Signs developed throughout the lungs, which, coupled with the height and the type of the temperature, pointed to the opinion that these organs had become the seat of a miliary tubercular deposit. A week or ten days before death unequivocal evidence of the condition having implicated the base of the brain supervened. The lung showed that the disease had begun at the base, and was probably of inflammatory origin; that this had probably become the infective centre for a general tuberculosis, primarily infecting the lung and secondarily the brain, for the latter presented numerous gray tubercles at its base. The only other organ which contained tubercles was the spleen. Dr Russell had carefully examined the lung and a crushed tubercle from the brain, and had not found the bacillus tuberculosis. Dr Russell also showed (*b.*) A PORTION OF THE BRAIN FROM A CASE OF TUBERCULAR MENINGITIS, which he had removed from a child of 11 months, who had been under the care of Dr Smith of Gorebridge. He showed it as a good specimen of tubercular disease of the base of the brain. The child had had otorrhœa, but he did not suppose there was any causal relationship between that and the condition of the brain found post-mortem.

2. *Dr MacGillivray* showed A PORTION OF LOWER JAW REMOVED FOR MALIGNANT TUMOUR. The operation was performed about a fortnight previously on a girl æt. 15. Three months before she had complained of pain in one of the back molars, which was extracted. The gum did not heal. When she was brought to hospital, there were a well-marked swelling of the jaw like an epulis, and a small ulcerated surface where the tooth had been removed. The swelling was blistered in hopes that it might be inflammatory, but did not give way to treatment. He therefore cut down upon it, and, finding the body of the bone affected, removed the angle and part of the ramus. A microscopic examination showed that the disease was a round-celled sarcoma.

3. *Mr Joseph Bell* showed A HALF TONGUE REMOVED FOR CANCEROUS DISEASE that day. The operation he had done was that known as Whitehead's, which consisted in successive snips or cuts with the scissors. The tongue was first split along the middle line and then divided close to its root on the affected side, the vessels being tied as they were cut or exposed. The hæmorrhage was very slight, the lingual artery being seized before it was cut. He thought the patient, an old man, had every chance of making

a good recovery. There was no external wound, the tongue being pulled well forward and easily got at.

4. *Dr James Carmichael* exhibited the LARYNX of a girl $3\frac{1}{2}$ years old, who was admitted on the 4th May into the Royal Hospital for Sick Children. She was in a hopeless state of asphyxia, and died six hours after admission. On examining her throat the fauces were covered with a grayish-white exudation, and from careful observation of the character of the breathing and physical examination of the chest, he expressed the opinion that the child suffered from membranous inflammation of the fauces, larynx, and trachea, and that the disease was probably spreading, as it usually does in diphtheria, into the bronchi. In these circumstances tracheotomy was not indicated. After death the opinion of the case which had been previously formed was corroborated, as the specimen now shown to the Society testified. The fauces, as would be seen, were covered all over with a sloughy purulent-looking membrane, which also covered both the upper and under surface of the epiglottis; this extended not only into the cavity of the larynx, which was almost occluded by it, but down the trachea as far as its bifurcation. The right and left bronchi were filled with sticky muco-purulent material. The mucous membrane beneath the exudation in larynx, trachea, and bronchi was swollen and congested; this was especially well marked over the arytenoid cartilages. On the pericardial surface of the heart were some small ecchymoses. Both lungs were congested and showed some emphysematous and collapsed patches.

5. *The President* showed—*First*, THE PHARYNX, GULLET, AIR PASSAGES, AND STOMACH of a case of poisoning with carbolic acid. The patient was found at 4 A.M. in a public park dead and lying on his face. He was last seen alive at 1 A.M., when he was intoxicated. Unfortunate in business, he had threatened to destroy himself. The appearances were highly characteristic of this active poison, and all the parts smelt strongly of the acid. *Second*, ULCERATION OF THE STOMACH, RUPTURE, AND PERITONITIS. The peculiarities of the case were,—1st, The situation of the ulcer, which was in the centre of the lesser curvature, and had given way in two places. The ulcer was the size of a florin. 2nd, The absence of vomiting and the remarkable latency of the symptoms of peritonitis. Deceased, who was fairly nourished, had suffered from dyspeptic symptoms for two years, and derived benefit from the use of charcoal. Recommended by his medical man to take change of air, he left London on Thursday night, 20th March, and arrived next morning in Edinburgh, went to his work, at which he was engaged the whole day. At 4 A.M., Friday, he woke his landlady, and complained of cramp in the stomach. Took hot water and afterwards castor oil with great benefit, but remained in

bed. At 8.30 he expressed himself as feeling well; at 9 P.M. he was found dead. There was no vomiting. *Third*, RUPTURE OF THE DIAPHRAGM. The rent was nearly 4 inches long, and was situated on the left side. Deceased was found at the side of the Water of Leith below the Dean, lying face downwards across a low wall which was covered with turf. His life was insured, and the question came to be, "Had he committed suicide by throwing himself over the bridge, or had he fallen accidentally into the water and been washed into the position in which he was found by the stream which had been in flood?" The post-mortem revealed this lesion, and the presence of the cardiac end of the stomach and the spleen in the left cavity of the chest. There was no other serious injury on the body. The friends were ultimately satisfied that the case was one of suicide. The President remarked on the importance of such specimens being placed for reference in museums, and referred to a case of injury to the diaphragm, in which Dr Matthews Duncan and himself had been anxious to see such injuries, but neither in the museum of the Royal College of Surgeons nor in that of the University were any to be found.

Dr Gunning remarked, with reference to rupture of the diaphragm, that once having an English horse out to grass he sent a boy for him. He was galloped in very hard, and fell dead at the door. A post-mortem examination showed this lesion, a rupture of the diaphragm.

III. ORIGINAL COMMUNICATIONS.

1. ON A CASE OF PATENT DUCTUS ARTERIOSUS, WITH ANEURISM OF THE PULMONARY ARTERY.

By JAMES FOULIS, M.D., Edinburgh.

MR PRESIDENT AND GENTLEMEN,—One morning, early in the year 1882, a young girl came to my consulting-room for advice for the following distressing symptoms:—Great palpitation of the heart on slight exertion, breathlessness, and buzzing noises in her chest, in her head and ears. She was very nervous, and her face bore an extremely anxious expression. The nose and lips were pale, with a slightly bluish tinge, and the conjunctival mucous membranes were very anæmic and flabby. There was no cough. Concluding from her pale face and lips that her symptoms were in a great measure due to bloodlessness, I made a very superficial examination at this time; but on placing the stethoscope over the base of the heart, I heard a loud blowing murmur, and at the same time felt a strong heaving impulse accompany the heart's systole. Immediately following the systolic murmur, another softer murmur was heard. These extraordinary murmurs puzzled me very much, as they were not at all like the ordinary humming murmurs of

anæmia. I at once prescribed iron and arsenic and rest, and obtained her promise that she would come back to me in three weeks time for further examination and advice, hoping by that time that her bloodlessness would have disappeared, and I should be better able to localize the murmurs and discover the nature of her cardiac affection. On her reappearance in three weeks time her face and lips showed that she had gained much blood, and that her general health had greatly improved; but on examining her heart at its base, to my intense surprise, I heard the murmurs louder than they were before, and much more marked in every respect. Her pulse was fairly good and quite regular. She still complained of palpitation on exertion, and her dyspnœa was little, if anything better; also the noises in her head and chest distressed her very much. On placing the stethoscope over the base of the heart, to the left side of the sternum, in the region of the second intercostal space, two loud murmurs were heard. The first murmur, which was very loud and blowing with a decided thrill in it, accompanied the systole of the heart, and at the same time a strong heaving impulse was communicated to the stethoscope. Just before this murmur ceased a distinct "click" was heard, and then immediately a second murmur, but of a softer nature and of shorter duration than the first murmur, was heard. On moving the stethoscope to the aortic area, the murmurs were still very distinctly heard, but the click which, as I have said, joined the two murmurs together was heard much more clearly, and I believe indicated the quick and sudden closure of the aortic valve. How the murmurs themselves were produced I could not at this time venture to say. They were heard also at the apex of the heart, but not nearly so loud as at the spot already stated. *The point of maximum intensity of both murmurs was to the left of the sternum between the second and third ribs.*

Upon entering into the history of my patient, I learned that about six years previously she had an attack of scarlet fever, but she could not say that her health in any way was worse after the attack. There was no history of rheumatic affection. I advised my patient to give up all unnecessary exertion, and to rest as much as possible, and to continue taking the iron and arsenic for another month.

At the end of a month I again saw her. She was looking greatly better. Her face and lips showed that her anæmia had quite disappeared. She still had great breathlessness on exertion, and complained much of the noises in her head and chest.

On listening over the base of the heart to the left of the sternum, in the second intercostal space, a strong heaving impulse was communicated to the stethoscope with the systole of the heart, and both murmurs, as already described, were heard with extraordinary clearness—I never heard anything like those murmurs before or since. At the apex of the heart the murmurs were not so loud as

at the spot indicated; and in the aortic area, while the click joining the two murmurs was very distinct, the murmurs themselves were but slightly diminished in intensity.

Being very much puzzled as to the cause of these murmurs, which, be it borne in mind, were much louder and better marked since the girl's anæmia had disappeared, I thought it would be interesting to hear what Dr George Balfour had to say as to the cause of the murmurs. After a careful examination he came to the conclusion that the case was one of patent ductus arteriosus, but at the same time confessed that his diagnosis was founded upon hypothetical rather than well-known scientific data. As regards treatment of the case, Dr Balfour advised that she should continue to take iron and arsenic, and should rest as much as possible.

From time to time I visited my patient at her own home, as she was unable to come to my house. At this time my friend Dr Wylie saw her with me. He also kindly took me to see a case in the Infirmary which was of great interest to the physicians. The case was believed to be one of patent ductus arteriosus. I had an opportunity of examining this girl immediately after seeing my own patient. On auscultation in the second left intercostal space two loud murmurs were heard very like those described as existing in my own case, but both murmurs seemed coarser and rougher, and the girl herself had a very healthy, even ruddy appearance.

The summer being at hand, my patient went into the country for change of air, and I did not see her for two or three months, but I heard from a medical friend that he had been called to treat my patient for hæmoptysis and bleeding at the nose. From this time her health began to fall off greatly; she suffered much from cough. Her feet were occasionally swollen, and she became very anæmic.

I heard from the girl's mother frequently to this effect,—that my patient suffered greatly from breathlessness and cough, and that she often coughed up some blood, and had had several attacks of bleeding at the nose, and was now extremely pale and weak. As she was rapidly getting worse, I advised her return to Edinburgh so as to be under my care.

She returned to Edinburgh on the afternoon of the 31st August, and I saw her at 8.30 P.M. She was then sitting up in a chair by the fireside looking extremely pale and ill. What struck me at once was the quickness of her breathing. Her pulse was 120, and respirations about 54 per minute. On examining the chest there were crackling, moist sounds on both sides in front and behind; there was constant cough, with some frothy and bloody expectoration. The veins in the neck were much distended. She could not lie down, but was kept in bed propped up with pillows. On placing the stethoscope over the base of the heart on the second intercostal space, a very strong heaving impulse was communicated

to the instrument, but, to my great surprise, both loud murmurs had almost entirely disappeared. The heart was violently wobbling about and striking against a large area of the chest wall, and occasionally I heard two sounds which I can best describe by the words "flupp," "flupp," pronounced quickly. How these sounds were produced I could not then say, but I came to the conclusion that all the cavities of the heart were extremely overgorged and in a state of tension.

The pallid face and bloodless and bluish lips showed that she was extremely anæmic and somewhat cyanotic. Day by day she lost more blood in the frothy expectoration. Her urine, which was very scanty, contained one-fifth albumen. She was at times delirious. She was not allowed to take any food but milk and water with white of egg. For some time she showed a decided improvement, but the distressing cough, with bloody expectoration, never left her. Some days better, some days worse, she lingered on until about the 20th September, when she began to complain of sharp cutting pain in the region of her heart; and on stethoscopic examination a loud, continuous, rough friction sound was heard all over the cardiac area. Pericarditis had evidently set in. After her return from the country I did not again hear the remarkable murmurs. They had entirely disappeared; but it was often possible to hear the "flupp," "flupp" sounds during the violently wobbling action of the heart. She died on the 25th September, thoroughly worn out and exhausted, 22 years of age.

Such is the very imperfect history of this case so far, but I am indebted to my friend Dr Wyllie for the following notes taken at his dictation at the post-mortem examination performed on the 27th September 1882:—

Description of Body.—The body was 5 ft. 1¼ inches in length. Circumference of body just below the level of the mammæ was 27¾ inches. The body was delicately formed, rather emaciated. Mammæ rather small. There were evidences of commencing putrefaction, abdomen being distended and tympanitic. Skin over the abdominal region being greenish blue in tint. The same tint of skin was seen on the throat. Rigor mortis still slightly present in the legs, but absent in neck and upper extremities. The legs below the knees were somewhat œdematous, pitting on pressure being best marked above the ankles.

On proceeding to examine the chest, the skin over the costal parietes was first reflected before the thoracic or abdominal cavities were opened into. The chest wall was then transfixed with four long barbed crotchet needles at the following points:—

Needle No. 1 was driven into the second left intercostal space, exactly at middle distance between the two cartilages, at a spot one inch to the left of the sternal margin.

Needle No. 2 was inserted in the same intercostal space exactly between the two ribs, but at the distance of ½ inch from the left margin of the sternum.

Needle No. 3 was inserted into the third left intercostal space, exactly at mid distance between the third and fourth ribs, at a spot $\frac{1}{2}$ inch from the left margin of the sternum.

Needle No. 4 was inserted in the second *right* intercostal space, mid way between the ribs, at a spot $\frac{1}{2}$ inch from the right margin of the sternum.

These needles, being thus fixed in position, were driven deeply inwards in a vertical direction, and the costal cartilages were divided in the usual way on each side, and the sternum with the attached cartilages and soft parts was removed. The needles, being fixed by their barbed points to the cardiac tissues, were left attached to the heart after the sternum had been removed.

On thus removing the sternum the needles which had been inserted to the left side of the sternal bone were all found to enter directly into the anterior surface of the pericardial sac without piercing the margin of the left lung, which lay close, $\frac{1}{4}$ inch, to the outer side of needle No. 1. On the other hand, the needle No. 4, which was inserted half an inch to the right of the sternum in the second intercostal space, transfixes the margin of the right lung in its upper lobe $\frac{3}{4}$ inch to the right of its free edge. Having thus transfixed the lung, this needle passed into the surface of the pericardium close to the right margin of the sac.

The *pericardium* was next opened. The sac was found to contain about $\frac{1}{2}$ pint of blood-stained serum, and the serous membrane, parietal and visceral, was found to be coated with a delicate villous layer of soft and recent lymph, which extended in patches over the whole surface of the heart and its containing sac.

The following was now found to be the positions of the needles:—

Needle No. 1 pierced the anterior surface of the pulmonary artery about its middle, and at a spot about $\frac{1}{4}$ inch above its valve.

Needle No. 2 transfixes the *aorta* immediately above its valve, passing immediately to the right of the pulmonary artery before piercing the *aorta*.

Needle No. 3 pierced the conus arteriosus 1 inch below the pulmonary valve, and at a spot $\frac{1}{4}$ inch to the right of the middle line of the conus arteriosus, and then passed backwards through the interventricular septum, and transfixes the left ventricle about an inch below the aortic valve.

Needle No. 4 passed through the pericardial sac at its outer limit, and merely transfixes a portion of the right wall of the heart without entering its chambers.

The Heart as a whole was much enlarged, measuring in length from pulmonary valve to apex $4\frac{3}{4}$ inches; and when flattened out, it measured in breadth across its middle $4\frac{1}{2}$ inches. Its muscular substance was a good deal softened by putrefaction. There was evident hypertrophy of both ventricles, the left wall measuring $\frac{5}{8}$ inch in thickness, and the right wall measuring $\frac{3}{8}$ inch in thick-

ness. The ventricular cavities were both considerably dilated. The auricles were not pierced by any of the needles. The left auricle was only slightly dilated, and its appendix was empty and flaccid, being in position behind and to the left of the pulmonary artery, and quite overshadowed by the aneurism on the pulmonary artery to be presently described. The appendix of the left auricle as a whole was large enough to contain easily the point of the forefinger, but the cavities of its fringe-like margins were not large enough to permit the entrance of the little finger point. The right auricle was considerably dilated, except as regards its appendix, which was of natural size.

The Lungs.—The right lung, especially at its upper lobe, contained a number of old and indurated infarctions of a grayish red colour. Both lungs were greatly congested and oedematous, with a feeling of partial consolidation, probably the result of catarrhal pneumonia.

The Liver was natural.

Spleen was greatly enlarged, measuring 8 inches in length by $4\frac{1}{2}$ in breadth.

The Kidneys were much congested and enlarged, and were softened by putrefaction; otherwise they were natural.

The heart, pericardium, and great vessels, were now removed and carefully dissected, without severing the connexion of the heart and its great vessels.

The following was found to be the condition of the great vessels:—

The Conus Arteriosus was very considerably dilated. When the pericardium was first opened its anterior surface was found bulging forward with unnatural prominence.

The Pulmonary Artery measured from the base of its valvular cusps to its bifurcation, $2\frac{5}{8}$ inches. At the bifurcation the ductus arteriosus communication with the aorta was patent; the orifice of communication being large enough to admit freely a goose quill.

The Ductus Arteriosus was represented by a very short vascular trunk about $\frac{1}{2}$ inch in length. When viewed externally its narrowest diameter appeared to be about $\frac{1}{4}$ inch. At its aortic attachment there was a bulging of the aortic wall, which corresponded with the funnel-shaped opening of the ductus arteriosus on the inner wall of the aorta.

The Pulmonary Artery, as a whole, was found to be dilated to at least twice its natural size, but this dilatation was not uniform in all directions, for whilst the posterior, right lateral, and anterior walls presented no special bulging or marked irregularity of outline, the left lateral wall from the ductus arteriosus close down to situation of the pulmonary valve was bulged in an outward direction towards the left, and at the lower limit, where the bulging was greatest, a regular aneurismal sac was formed about the size of a large walnut. This sac was contained within the pericardium, and, as already said, lay in front of the left auricular appendix, which it entirely hid from view.

The needle No. 1 transfixed the middle of the pulmonary artery, about $\frac{3}{4}$ inch to the right of the aneurismal sac. The sac was filled internally with pretty firm coagulum, and from the sac this coagulum extended along the surface of the left anterior wall of the artery to near its bifurcation, being bound to this wall by pretty firm adhesion. A conical prolongation of this clot partially blocked the lumen of the ductus arteriosus. Externally, the part of the sac which was contained within the pericardium was covered with abundant inflammatory lymph of older date than the lymph effused on the pericardial surface generally. This lymph was deeply stained with blood colouring matter, and formed firm adhesions between the aneurismal sac and the parietal layer of the pericardium.

The primary divisions of the pulmonary artery were both of large size, the right being at least nearly twice as large as usual. Into the left division there projected for about half an inch an offshoot of the coagulum above described as existing in the main trunk of the pulmonary artery. This, however, occupied only about one-fourth or one-sixth of the lumen of the vessel.

THE VALVES OF THE HEART.

The Pulmonary Valve was extensively diseased. Each cusp on its ventricular surface was covered with ragged vegetations, attached for the most part to the neighbourhood of the corpus Arantii and to the margin and upper half of the cusp. These vegetations were of soft granular consistence, and appeared to be made up in part of true vegetative growths and in part of secondary fibrinous deposit. The anterior cusp was the least diseased, retaining its full size, but presenting a large amount of vegetations in the neighbourhood of the corpus Arantii. The two posterior cusps, right and left, were both reduced to about half their natural dimensions by ulceration or atrophic change, and were fringed along the whole length of their free margins with large and ragged vegetations of soft consistence. The ragged and vegetating edges of these two cusps were both turned over so as to hang as it were dependent towards the ventricular cavity, and in the case of the anterior cusp it was evident from the dipping of its free margin that there had been a tendency to a similar state of matters.

The circumference of the pulmonary orifice at the level of its cusps was $3\frac{1}{2}$ inches.

The Aortic Valve.—The orifice of the aorta was about natural in size. All the cusps of the aortic valve, however, presented on their ventricular surface vegetations attached to the corpora Arantii. In the case of the anterior cusp the group of vegetations formed a mass the size of a small pea. On the other cusps the vegetations were much more minute, being about the size of small pin heads. The cusps otherwise retained their natural size and structure.

Fig I

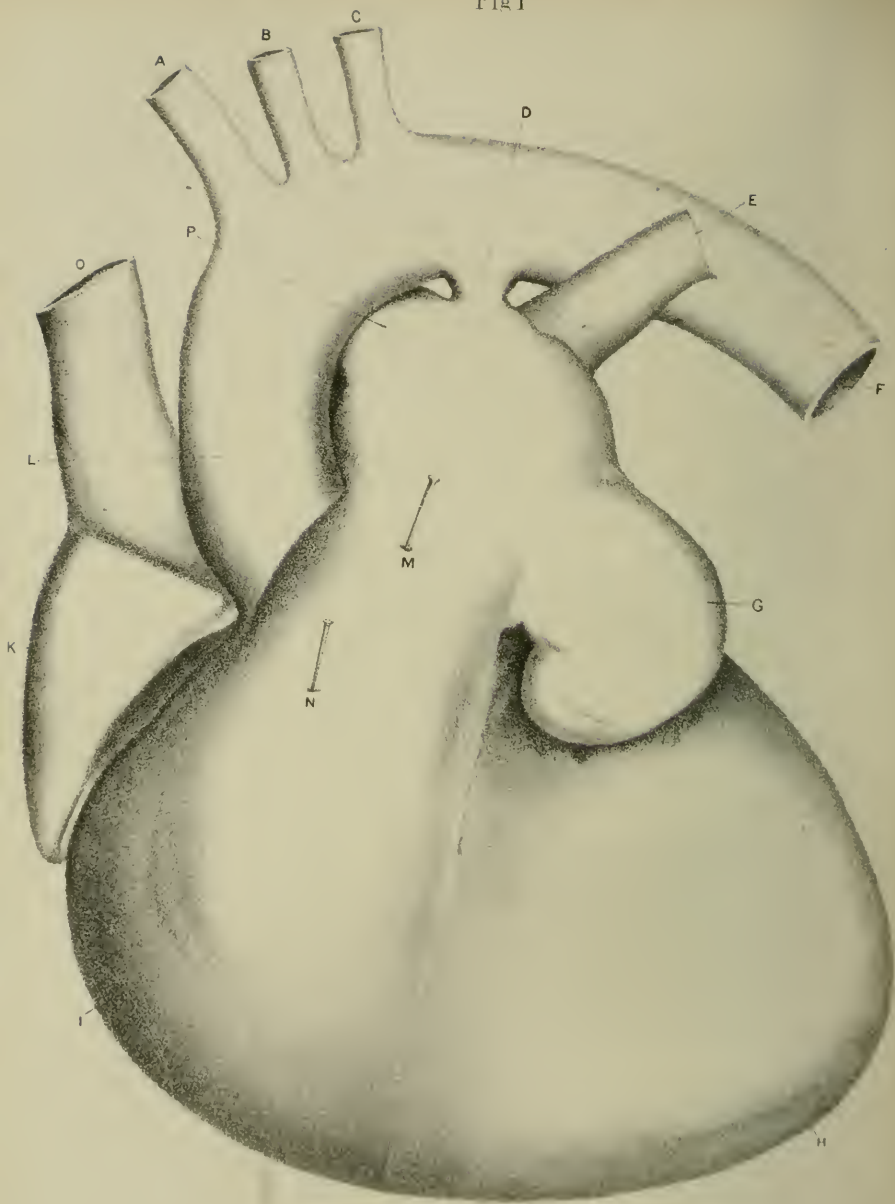
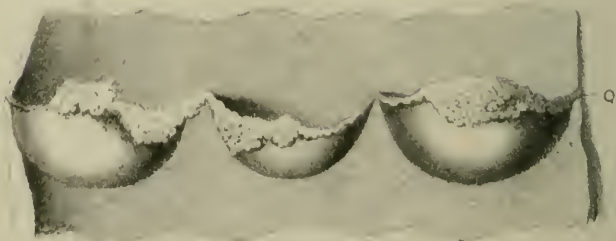


Fig II



The Mitral Valve.—The anterior or aortic cusp was well developed, and was natural, except that on its auricular surface, close to its free margin, it presented an attached mass of dense vegetation about the size of a small bean.

The Aorta was of natural size, both in its arch and descending portion. At the junction of the second and third portions of the arch, external to the line of the left subclavian artery, was the patent opening of the ductus arteriosus already referred to. The margins of the opening were smooth, and the wall of the aorta at the point of junction with the ductus arteriosus was distended somewhat in a funnel-like manner. The lumen of the ductus arteriosus was large enough to transmit a goose quill, and was partly plugged by a protruding offshoot of the coagulum from the pulmonary artery, as already said. On the posterior wall of the aorta, *exactly opposite the orifice of the patent ductus arteriosus*, there was a rounded bulging or lens-shaped concavity in the aortic wall, which measured one inch in diameter, and in depth about half an inch. The cavity thus formed was partly filled up with adherent coagulum, and the limiting wall of the cavity was here hardened as if by atheromatous changes.

The Mitral Orifice was slightly dilated.

The Tricuspid Orifice was almost natural in size, and its cusps were quite normal.

The Foramen Ovale was quite closed.

The same letters when present in both Plates point to the same parts.

PLATE I.

FIG. 1.—Shows the heart with its large bloodvessels. All the cavities of the heart and the large bloodvessels were stuffed with wet cotton wool, so as to distend them. In this way the dilated right ventricle, conus arteriosus, and pulmonary artery are well shown. The descending portion of the aorta has been raised up from its normal position, so as to bring more clearly into view the ductus arteriosus.

- A, The innominate artery.
- B, Left carotid artery.
- C, Left subclavian artery.
- D, The patent ductus arteriosus.
- E, Left branch of the pulmonary artery.
- F, Descending portion of the aorta.
- G, Saccular aneurism, filled with granular clot, on the left anterior wall of the pulmonary artery.
- H, Dilated left ventricle.
- I, Dilated right ventricle.
- K, Right auricle.
- L, Ascending aorta.
- M, This letter is placed below the needle No. 1, which was inserted into the second left intercostal space, midway between the ribs, at a spot 1 inch to the left of the sternal margin. It pierced the pulmonary artery at its middle, about $\frac{1}{2}$ inch above its valve.
- N, This letter is placed below the needle No. 3, which was inserted into the third left intercostal space, midway between the ribs, at a spot $\frac{1}{2}$ inch to the left of the sternal margin. It pierced the dilated conus arteri-

osus $\frac{1}{4}$ inch to the right of the middle line, and 1 inch below the pulmonary valve.

O, Superior vena cava.

P, Right branch of pulmonary artery.

FIG. 2.—The diseased cusps of the pulmonary valve.

Q points to the anterior cusp of the valve.

PLATE II.

FIG. 3.—Shows the heart so dissected as to present to view the right ventricle, conus arteriosus, and pulmonary artery as one tube. A portion of the wall of the right ventricle, conus arteriosus, and pulmonary artery has been removed.

R points to the granular clot filling the saccular aneurism (G) on the left anterior wall of the pulmonary artery. A conical prolongation of this clot was found plugging the ductus arteriosus, and another prolongation of the clot was found partially obstructing the left branch of the pulmonary artery.

S points to a large pin which was inserted into the lumen of the patent ductus arteriosus.

T points to the anterior cusp of the pulmonary valve.

FIG. 4.—Shows a portion of the ascending, transverse, and descending aorta, seen from behind.

X A distinct aneurismal bulging out, situated on the posterior wall of the aorta, just external to the line of the left subclavian artery, and immediately opposite to the aortic opening of the ductus arteriosus.

After hearing this description of the post-mortem examination, the first question which naturally suggests itself is, How were the murmurs which I have described produced? But before attempting to answer this question, let us shortly recapitulate the lesions which were found at the post-mortem examination in the heart and great bloodvessels. Let us look at these lesions following the course of the circulation of the blood.

The right auricle was greatly dilated. The tricuspid orifice was very slightly dilated, but as far as could be seen the tricuspid valve was competent. The right ventricle was much dilated and its wall thinned. The conus arteriosus was greatly dilated and its wall thinned. The pulmonary valve was very extensively diseased in its three cusps, each cusp being atrophied and shrunken, and covered over with granular vegetations on its ventricular side. The pulmonary artery itself was greatly dilated, and its left anterior wall converted into a saccular aneurism the size of a walnut, which was filled with granular clot. The ductus arteriosus once patent, allowing a goose quill to pass through it, was now partly plugged with clot. The left auricle was almost natural. The mitral valve was natural, except that its anterior cusp on the *auricular* surface presented a mass of vegetation the size of a small bean. The left ventricle was much dilated and the wall hypertrophied. The aortic valve was competent, but on its ventricular surface the anterior cusp presented a mass of dense vegetation the size of a small pea, while on the other cusps were small vegetations the size of pin heads.

Fig III

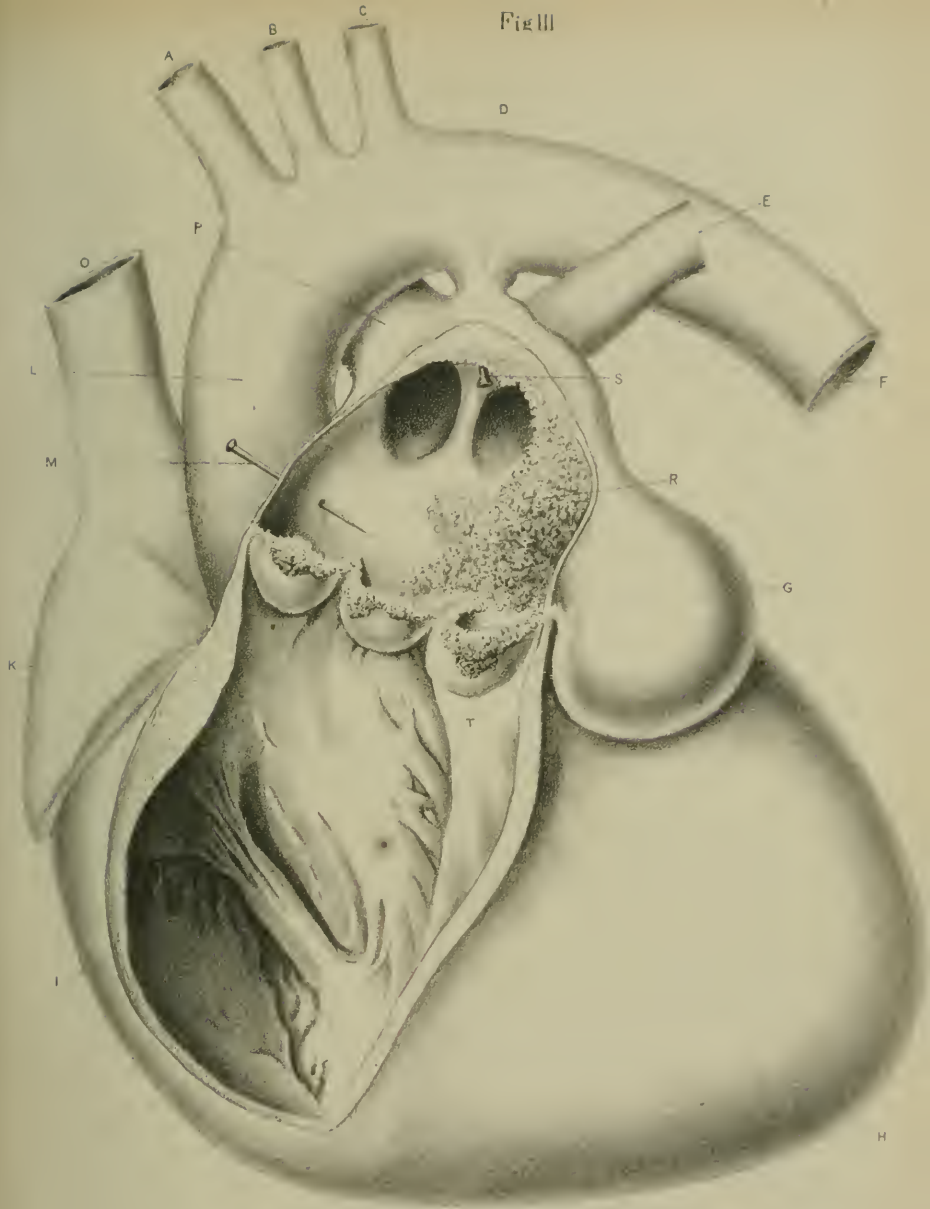
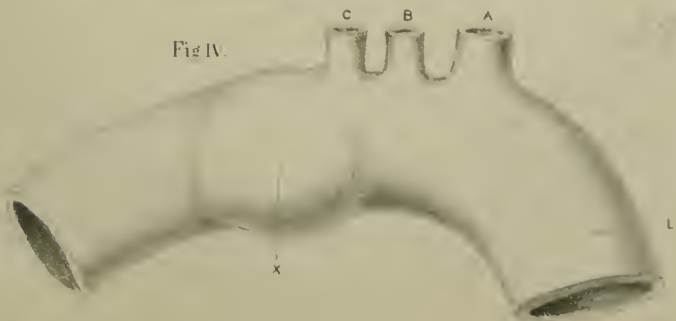
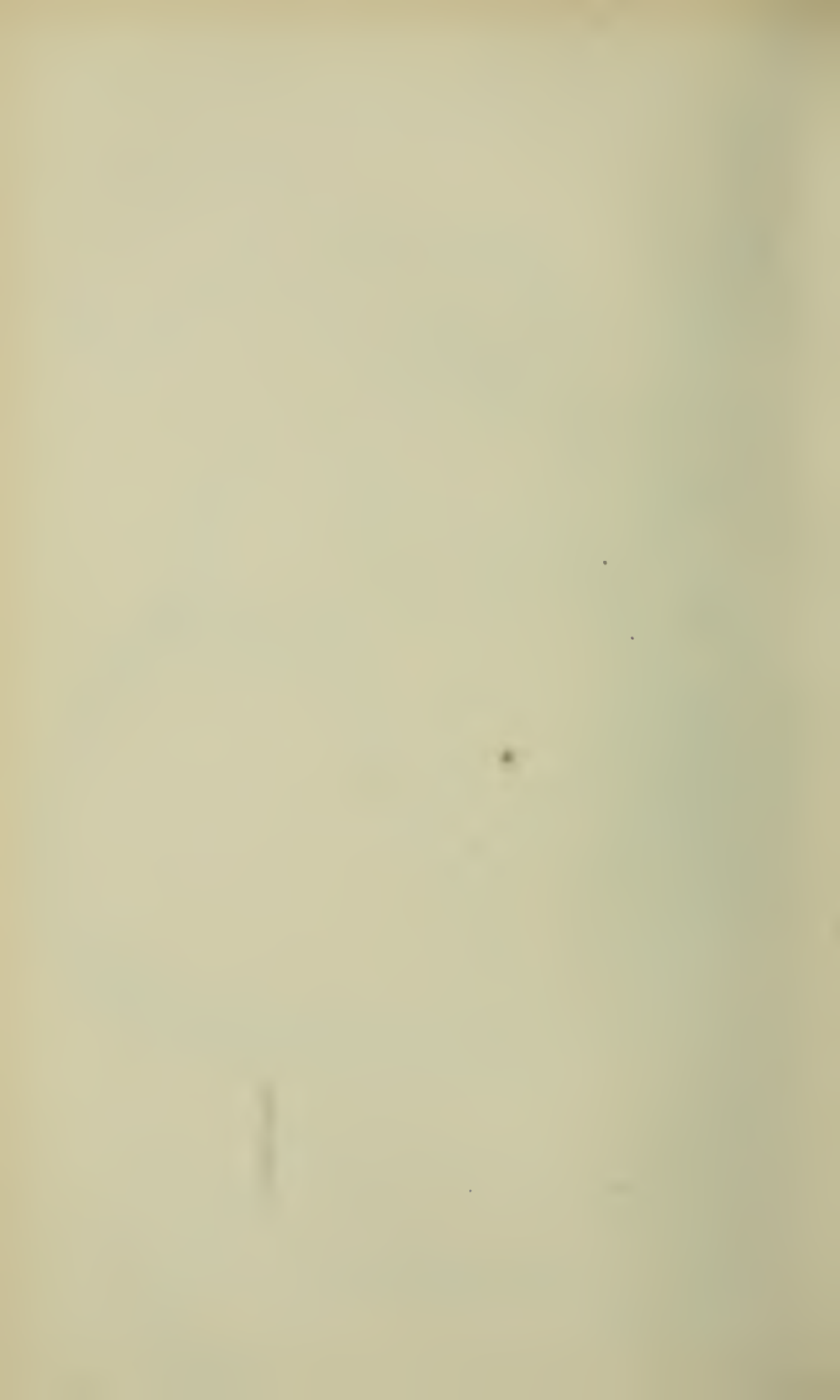


Fig IV.





Let us also remember that the needle No. 1, which was driven into the second left intercostal space at a spot one inch to the left of the sternal margin, pierced the pulmonary artery in its middle, just about $\frac{1}{4}$ inch above the diseased pulmonary valve, and that on stethoscopic examination of the heart, the murmurs referred to were heard with greatest intensity at this very spot.

Now, during the simultaneous contraction of the two ventricles, blood driven through the aortic valve and through the pulmonary valve would come in contact with cusps whose margins were rough with dendritic vegetations. In the case of the aortic valve it is certainly doubtful if a murmur was produced there, but in the case of the pulmonary valve, where the cusps were so extensively diseased, each cusp being shrunken and covered over on its ventricular surface with masses of vegetations, there can be little doubt, I think, that a loud murmur was produced at that situation. If any murmur was produced at the aortic valve, I doubt if it was possible to hear it in the presence of the much louder murmur at the pulmonary valve. A careful examination of the aortic valve showed that it was competent, and that the pea-sized vegetation on the ventricular side of its anterior cusp would offer very little obstruction to the flow of blood through the valve. Hence I doubt if any murmur was produced at the aortic valve.

With regard to the mitral orifice,—if any murmur was produced there during the systole of the heart, it must have been a very feeble one, which could not be distinguished in the presence of the loud murmur at the pulmonary valve. The first loud murmur heard with such remarkable clearness in the pulmonary area could be very clearly heard at the apex of the heart.

I have already said that at the end of the first loud blowing and thrilling murmur a distinct click was heard. This click was clearly heard over the aortic area, and I believe was caused by the sudden closure of the aortic valve under great pressure. Did the blood rushing through the patent ductus arteriosus during the heart's systole contribute anything towards the first loud and thrilling murmur? The ductus arteriosus is situated much deeper in the chest than the valve of the pulmonary artery, but it is likely that any murmur produced at that patent orifice would be most clearly heard at the spot where the pulmonary artery is nearest to the chest wall, *i.e.*, in the second intercostal space to the left of the sternal margin. It appears to me quite impossible that such a volume of blood as would be driven by the contraction of the right ventricle through the patent ductus arteriosus could pass through that orifice without distending it, and causing its margins to vibrate. That the blood was being driven through the patent ductus arteriosus with much force up to within three weeks of the girl's death was evident from the fact that there existed on the posterior wall of the aorta, just external to the left subclavian artery,

an aneurismal dilatation or bulging out of that wall, which could have been only caused by a column of blood driven with much force against the wall in that situation; and this bulging out of the posterior wall of the aorta was exactly opposite the aortic orifice of the patent ductus arteriosus. The ductus arteriosus in the fetus is as large as the pulmonary artery itself, and I don't suppose that a murmur is produced by blood rushing through it during the foetal circulation; but when it is reduced to a tube which just admits a goose quill, and has a powerful right ventricle behind it, the quantity of blood which is driven through it must distend it and give rise to a vibratory or thrilling murmur at its margins.

With regard to the second murmur, which I have described as beginning immediately after the closure of the aortic valve, we have to look to the tricuspid orifice, to the mitral orifice, and to the ductus arteriosus for the explanation of its production. Bearing in mind that the tricuspid orifice was healthy, and its valve competent, and that the aortic valve was competent, little or no regurgitation could take place through those valves. Now, when the two ventricles contracted the blood driven by the hypertrophied left ventricle would be sent with much force into the aorta, and at the same time, by the contraction of the right ventricle, a considerable quantity of blood would be driven through the patent ductus arteriosus, also into the aorta, and as a result that vessel would be greatly distended. According to the well-known law, it would recoil with the same force with which it had been distended. The aortic valve being competent, a certain quantity of blood would be driven back with much force into the pulmonary artery during the recoil of the aorta; and supposing the pulmonary valve was competent, a great distending force would be exerted, according to Pascal's law, upon the inner wall of the pulmonary artery and on the pulmonary valve; and if this valve was incompetent, then the distending force would be exerted on the inner wall of the conus arteriosus and of the right ventricle, as well as upon the inner wall of the pulmonary artery.

It appears to me that the aneurismal sac on the pulmonary artery was produced in one of the two following ways:—Either the recoil current through the ductus arteriosus, by distending the pulmonary artery in all directions, gradually caused that vessel to dilate and to give way at one part, say its weakest part, until it was bulged out in the form of a sac, or the recoil current impinging directly on one part of the wall of the pulmonary artery with much force, gradually bulged out that part until the aneurismal sac was produced. It is interesting to note that if the axis of the lumen of the ductus arteriosus was prolonged, at one end it would strike the middle of the saccular aneurism on the pulmonary artery, and at the other end would strike the middle of the aneurismal dilatation on the posterior wall of the aorta, which I have described. From

which observation it is only fair to conclude that both these aneurismal dilatations were produced in the same way, namely, by the distending force of the to and fro current, driven through the patent ductus arteriosus, acting on the vessels at the spots where the aneurisins were produced. There can be no doubt that as a result of the diseased state and incompetency of the pulmonary valve, a very great distending force must have been exerted upon the pulmonary artery, the conus arteriosus, and the right ventricle at one and the same time—all these structures, as one tube, being greatly dilated.

From the fact that the left auricle was very slightly dilated, it is only fair to conclude that there was very little obstruction, if any, to the flow of blood through the mitral orifice during the auricular systole; and although a good-sized vegetation was found on the auricular surface of the aortic cusp of the mitral valve, it was quite evident that there was no obstruction to the flow of blood through the valve, as the auriculo-ventricular orifice was dilated rather than constricted. I believe, therefore, that no murmur whatever was produced at the mitral orifice during the auricular contraction. We have then only the pulmonary artery and the patent ductus arteriosus to account for the production of the second murmur. As the result of careful observation, it was noted that the second murmur began immediately after the closure of the aortic valve, and was heard most distinctly in the second intercostal space to the left of the sternal margin. Was it produced by the blood rushing back through the ductus arteriosus during the powerful recoil of the aorta, or was it produced by regurgitation of blood through the diseased pulmonary valve? It was quite evident from a careful examination of the axis of the tube of the ductus arteriosus, and of the situation of the pulmonary valve, that the recoil current through the ductus arteriosus would impinge not on the cusps of that valve, but on a spot at least an inch away from them, outwardly towards the left, that spot being the centre of the saccular aneurism on the pulmonary artery. The rush of blood back through the patent ductus arteriosus mixing with the blood in the distended pulmonary artery, conus arteriosus, and right ventricle, would set up a commotion in that fluid, but I fail to see how the cusps of the diseased pulmonary valve could be set into a state of vibration sufficient to cause a murmur by the recoil current which would strike, not on them, but on the dilated wall of the pulmonary artery, fully an inch above them. It must be also borne in mind that the pulmonary artery at the situation of its valvular cusps was greatly dilated, its circumference being at this spot $3\frac{1}{2}$ inches. The incompetency of this valve was therefore marked in the highest degree. The post-mortem examination also showed that its cusps were much shrunken and atrophied, and that their ragged margins hung dependent into the ventricular cavity. These ragged edges would undoubtedly be thrown forward and caused

to vibrate by the large volume of blood from the right ventricle rushing past them during the systole, after the closure of the right auriculo-ventricular valve. The fluid contents of the large right ventricle would have to be *squeezed* through the comparatively narrow gate of the pulmonary valve, and the ragged margins of that gate would vibrate in consequence. But it appears to me the case is quite different when we come to consider what took place during the rush of blood back through the ductus arteriosus. Granting that this recoil current did strike on the cusps of the pulmonary valve, in consequence of the *smallness* of its volume as compared with the great incompetency of the pulmonary valve, there would be very little obstruction to its regurgitant flow into the right ventricle. I therefore think that whatever regurgitant murmur was produced at the seat of the pulmonary valve was, comparatively speaking, nothing in the presence of the loud murmur produced at the orifice of the ductus arteriosus during the recoil of the aorta.

It is quite clear that a large quantity of blood could not possibly be pumped back through the patent ductus arteriosus without causing that tube to dilate, and as a result the blood rushing past the distended margins of the tube would set up a vibration of these margins and thus give rise to a murmur.

I have therefore come to the conclusion that the first murmur really consisted of two murmurs, the one being produced by the rush of blood past the diseased cusps of the pulmonary valve, and the other being produced by the blood rushing through the distended ductus arteriosus, both together constituting the loud blowing and thrilling murmur which accompanied the systole of the ventricles. The second murmur, I believe, was produced by the blood rushing back through the patent ductus arteriosus during the recoil of the aorta.

It cannot have escaped your recollection that on the return of my patient from the country, on the 31st August 1882, she was in a state of great danger from extreme congestion of the lungs. The whole venous system was greatly engorged, and the urine was loaded with albumen. She had constant cough with frothy and bloody expectoration; she died within a month after her return. An examination of the lungs after death, especially the right lung, showed that for a considerable time previous to her death hæmorrhagic infarcti had been forming in those organs—the right lung throughout a great area being rendered useless in consequence. There can be no doubt that these infarcti had formed as the result of embolic plugging of the terminal branches of the pulmonary artery by little pieces of vegetations detached from the extensively diseased cusps of the pulmonary valve, or by little fragments of coagulum detached from the granular clot filling the aneurism on the pulmonary artery. The post-mortem examination also showed that the ductus arteriosus was almost completely plugged by a conical clot which projected from the clot in the

saccular aneurism on the pulmonary artery, and that the large left branch of the pulmonary artery was greatly obstructed by a clot from the same source. Did these conditions exist when my patient returned from the country on the 31st of August, more than three weeks before her death? The history of the case shows that she began to be extremely ill a day or two before her return on the 31st August. When I saw her on that day her dyspnoea was most distressing. The venous system was everywhere terribly engorged; the veins of the neck being greatly distended. The urine contained a very large quantity of albumen, about one-fifth. There was crepitation in all parts of her lungs. She had constant cough with bloody and frothy expectoration; and from this time on to her death there were frequent attacks of hæmoptysis and epistaxis. The heart all the time was heaving against the wall of the chest with a remarkably strong impulse, and, strange to say, the two loud murmurs had entirely disappeared; but during the violently wobbling action of the heart one could hear the two sounds "flupp," "flupp" quite distinctly. Her extremely distressing symptoms evidently began as the result of the plugging of the terminal branches of the pulmonary artery, and as the result of the obstruction in the left branch of the pulmonary artery, and the consequent obstruction to the circulation through the lungs.

How is it possible to account for the disappearance of the two loud murmurs and for the production of the sounds "flupp," "flupp?"

If my explanation of the production of the two loud murmurs is correct, I think their disappearance may be accounted for in the following way:—Bearing in mind that numerous terminal branches of the pulmonary artery in both lungs were plugged, and that one of the main branches of the pulmonary artery itself was greatly obstructed, and that both lungs were in a state of catarrhal pneumonia, and that the kidneys and spleen were greatly congested—the whole forming an impediment to the circulation of blood through the lungs which the distended and dilated right side of the heart could not overcome—it would necessarily follow in consequence of this engorged state of the whole right side of the heart, that too small a quantity of blood would be driven past the cusps of the pulmonary valve to set them in vibratory commotion. Hence the absence of the first part of the loud systolic murmur. But in addition, the almost complete closure of the ductus arteriosus by a conical clot from the aneurism would effectually prevent the passage of a sufficient quantity of blood through that opening during the heart's systole as would cause its margins to vibrate. Hence the disappearance of the second part of the loud systolic murmur. In the same way this plugging of the once patent ductus arteriosus would equally prevent the rush back of blood during the recoil of the aorta. Hence the disappearance of the second loud murmur.

With regard to the production of the sounds "flupp," "flupp," heard over the base of the heart, it was quite evident that the first was caused by the reduplication and accentuation of the first sound of the heart with the systole, and that the second shorter sound was caused by the rapid closure of the aortic valve—the whole arterial vascular system being in a state of great tension due to the block to the circulation in the right side of the heart and in the lungs.

Gentlemen, after hearing this paper so far you will ask, Have we learnt anything, either from the symptoms observed during life or from the post-mortem appearances, which would enable us in the future accurately to diagnose a case of patent ductus arteriosus with aneurism of the pulmonary artery?

The chief facts in connexion with the physical symptoms observed during life which I have brought before you are these:—

On auscultation a loud, blowing, thrilling murmur accompanying the ventricular systole, was heard with maximum intensity in the second left intercostal space, fully an inch to the left of the sternum, and at the same time a strong heaving pulsation was communicated to the stethoscope at this spot. The best anatomical authorities state that the left border of the pulmonary artery lies in the second intercostal space, at a distance of three-fourths of an inch to the left of the margin of the sternum. If we meet with a case in which a set of symptoms, such as those just described, exist, we may fairly conclude that there is a considerable dilatation of the pulmonary artery present. A dilated pulmonary artery alone would not, however, give rise to such an extraordinary murmur as was heard with the ventricular systole in my patient's case. Dilatation of the pulmonary artery above its valve is such a rare thing in a young person, that we must look for some special cause of obstruction *in front* of the dilatation to account for it. In connexion with a dilated pulmonary artery a murmur may or may not be present. Its presence with the ventricular systole will depend on whether the pulmonary valve is diseased or not, or whether there is some obstruction to the flow of blood through the pulmonary artery at a point farther on than the seat of the pulmonary valve. So rare is the existence of a dilated pulmonary artery in a young person that if we find an example of it we may suspect that a patent ductus arteriosus is the cause of it, because such an obstruction in the lungs or in the pulmonary artery itself, apart from a patent ductus arteriosus, as would cause a great dilatation of the pulmonary artery, is almost unknown.

An acquaintance with Pascal's law of the diffusion of pressure in fluids will at once enable us to understand how even a strongly walled pulmonary artery may gradually become dilated into a saccular aneurism, if there is a leak back from the aorta into the pulmonary artery, as exists in the case of a patent

ductus arteriosus. The short wide tube of the pulmonary artery above its valve is exposed to a very great extensile pressure by the column of blood escaping back through that patent orifice acting on the fluid contents of that artery. Dilatation once begun, the extensile pressure gradually becomes greater and greater, until the tube is burst or a saccular aneurism is produced. But not only is this pressure exerted on the inner wall of the pulmonary artery, but the cusps of the pulmonary valve are exposed to similar pressure. Sooner or later they give way, and become diseased and incompetent.

There can be no doubt, then, that by far the commonest cause of an aneurismal dilatation of the pulmonary artery is the existence of a patent ductus arteriosus. When this exists, a loud thrilling murmur will be heard accompanying the ventricular systole caused by the rush of blood through that orifice into the aorta, and in the same way after the closure of the aortic valve, another murmur would be caused by the rush of blood back through that orifice during the recoil of the aorta, and this rush back current is the one which gradually causes the dilatation of the pulmonary artery.

If, then, we meet with a case in which there is no history of lung disease such as would cause obstruction to the flow of the circulation through the lungs, and if on auscultation in the second left intercostal space we find there is a heaving pulsation communicated to the stethoscope with the ventricular systole, and at the same time a loud thrilling murmur is heard with maximum intensity at this very spot, and also heard in lines radiating upwards and outwards from this spot, and if this murmur is followed by another, similar in character, immediately following the closure of the aortic valve, we may fairly conclude that the cause of the aneurismal dilatation of the pulmonary artery, and of the two thrilling murmurs, is a patent ductus arteriosus.

These murmurs and the dilatation of the pulmonary artery may exist without any co-existing disease of the pulmonary valve. In fact, I should think that freedom from disease and competency of that valve was a necessary condition for the production of any marked aneurismal dilatation of the pulmonary artery itself. No doubt the valve must sooner or later become incompetent, and then the whole right side of the heart becomes subjected to great extensile pressure, and dilatation results, as was so well observed in the right side of the heart of my patient.

I do not believe that the attack of scarlet fever from which my patient suffered six or seven years before she came to consult me had in any way affected the right side of her heart. She was able to do hard work, even to dance, up to within a comparatively short time before I first saw her. We know that even a slight cold in such a case would be sufficient to throw much pressure on the pulmonary valve, and my belief is that her pulmonary valve began

to give way, and became crumpled and diseased as soon as embolic plugging of the terminal branches of the pulmonary artery took place.

From a careful examination of the aneurism and of the pulmonary valve, I think there can be no doubt that the former existed a long time before the latter began to be diseased and incompetent, and it was only when the latter became so that the distressing symptoms commenced which ultimately carried off my patient.

The mere presence of a patent ductus arteriosus and the consequent intermingling of venous and arterial blood does not seem to act very injuriously on the health of the patient. In my patient's case there was very little cyanosis at the first, and all her distressing symptoms resulted from the extreme dilatation of the whole right side of the heart, and the obstruction to the flow of blood through the lungs.

With regard to the literature of this subject there is very little to quote from or refer to. As far as I have been able to ascertain, there is no case recorded in this country in which a post-mortem examination has brought to light the pathological conditions which have been described in this paper.

In the *Glasgow Medical Journal* for 1879, Dr Wood Smith of Glasgow has a short paper, with a woodcut, on "A Probable Case of Aneurism of the Pulmonary Artery and Ductus Arteriosus," but the case was by no means clearly made out. The patient is still alive and in good health. In vol. xvii., *Archiv der Heilkunde*, Dr Lüttich has a case entitled "Fall von Aneurysma des Ductus Bottali und Thrombose der Aorta," and in Rokitan-sky's *Krankheiten der Arterien* there are several illustrations of cases of patent ductus arteriosus, but none of them resemble the case I have brought before you this evening.

I cannot close this paper without expressing my deep gratitude to Dr Wyllie for the careful manner in which he performed the post-mortem examination in this case, and for his accurate description of the pathological conditions found in connexion with the heart and large bloodvessels, and I am the first to admit that the value of this paper consists in the scientific accuracy of his descriptions.

The President said he was sure the Society would allow him in their name to thank Dr Foulis for his able and instructive paper, and for his admirable manner of illustrating it. He would call on Dr George Balfour to offer some remarks on this very rare lesion.

Dr Balfour said that by Dr Foulis's kindness he had had the opportunity of seeing this case during life, and as Dr Foulis very correctly stated, though he gave it as his opinion that the patient

had a patent ductus arteriosus, he by no means felt quite certain about it, as he had never had an opportunity of observing previously a similar case post-mortem. All cardiac cases were rendered doubly interesting from the varied character of the results which followed similar lesions. This was particularly the case in reference to congenital defects. Serious results often followed slight imperfections. A trifling patency of the foramen ovale might cause death rapidly, while, on the other hand, great defects might lead to little or no result. An entire absence of the septum ventriculorum might occur and exist for many years, the subject of the malformation doing well, and dying ultimately, perhaps, of some other disease or accident, as occurred in a case he had seen. They were all obliged to Dr Foulis and Dr Wyllie for the care with which they had placed this case before them. He now saw his way pretty clearly to make a diagnosis of patent ductus arteriosus during life. He had seen four cases in which he had given as his opinion that there was a patent ductus arteriosus, but this was the first in which his opinion had been verified. The first case he saw with the late Dr Warburton Begbie eleven years ago. In her case there was very faint pulsation between the second and third ribs on the left side, a loud systolic murmur, with vibratory thrill loudest in that position, propagated into the arteries, and extending down the aorta, and lost about the iliac arteries. In her case there was considerable cyanosis of the face and all the extremities, which were purple, cold, and puffy. Dr Begbie had seen her with his father ten years previously, and both had arrived at the same conclusion as himself. Prof. Sanders also saw her. He (Dr Balfour) had seen her since. The cyanosis was diminished. The thrill and murmur still continued, but were not so marked. The condition might be undergoing a cure by thrombosis of the duct, and if the thrombosis did not go too far it would be a perfect cure. The next case was that recorded by Dr Wood Smith, which had been under his (Dr B's.) care in the Infirmary. The pulsation was very distinct in her case, and the murmur was entirely similar—a loud thrilling vibratory murmur between the second and third ribs. He had taken this case with considerable care, but the book in which it was recorded had disappeared. Dr Wood Smith had since informed him that the girl had gone to Liverpool and was married. The third case was one to which Dr Foulis had referred as having been in the Infirmary at the time he (Dr Balfour) had left it. The lesions resembled much more closely those of Dr Foulis's case than either of the other two, the loud vibratory murmur, mainly systolic, which was conveyed through every artery of the body, and was accompanied by a peculiar metallic clang. The murmur extended beyond the click of the second sound, and there was probably some lesion of the pulmonary valves. The girl had not come in for her cardiac affection, but for scoliosis and weakness of the back. The cardiac condition was

only discovered accidentally. These cases were not susceptible to any treatment beyond rest and blood tonics.

Dr Wyllie said he differed slightly from *Dr Foulis* on the question of the production of the second bruit. The first bruit had a double origin. He should be inclined to think that the second had also a double origin.

Professor Grainger Stewart said he had greatly enjoyed this paper as an admirable statement of a rare case. In his own experience he could only recall one case that corresponded with it. He was of opinion that there was a patent ductus arteriosus, but there was no post-mortem to confirm it. A gentleman consulted him about five years ago, after having been advised to give up his professional duty because of hæmoptysis and cardiac murmur. He (*Prof. Stewart*) was satisfied on examination that the cardiac lesion was congenital, and probably, as he had said, a patent ductus arteriosus. He ventured to advise the patient to resume work, which he did. Little more than a year ago he saw the gentleman in Cannes dying of pulmonary phthisis. He died afterwards at home, but post-mortem examination was not allowed. He was inclined to agree with *Dr Wyllie* as to the causation of the second bruit. He thought that if there was thickening of the tricuspid valve to help towards the production of the first bruit there must have been incompetence enough to allow of the second. He should like to ask *Dr Foulis* if he did not think that the patient died of ulcerative endocarditis. This would account for the albuminuria, for the lesions in the lungs and spleen, for the vegetations on the valves roughened and breaking down.

Dr Gibson said that in common with the other members he felt greatly indebted to *Dr Foulis* for the careful way in which he had brought this case before them. There was one point on which he felt some difficulty. *Dr Foulis* was evidently of opinion that the blood current passed backwards and forwards through the patent ductus arteriosus. Now, the pressure in the aorta was much greater than in the pulmonary artery. He did not see how, in accordance with physical laws, the blood could flow from the pulmonary artery into the aorta.

Dr Wm. Russell said that the idea he had formed of this case was that in the earlier part of its history the murmurs had to be accounted for quite apart from the vegetations found after death. He would submit that in the latter part of the case, when the murmurs had disappeared, the condition was one of vegetative endocarditis, as suggested by *Prof. Stewart*. He should like to ask if the systolic murmur could not be accounted for by the presence of aneurism on the pulmonary artery. With *Dr Gibson* he thought that the current of blood would not go from the pulmonary artery into the aorta; on the contrary, the current would naturally go from aorta into pulmonary artery. As *Dr Foulis* lucidly brought out, the current impinged right on the centre of the

aneurism on the pulmonary artery. The diastolic murmur, at least, was probably produced in this way. Was not the aneurism on the pulmonary artery sufficient to produce a systolic murmur without calling in anything else to explain it? He had asked Dr Foulis whether the temperature was elevated after the patient's return from the country. It was, and this, coupled with the other conditions, showed that the condition was very different from what it was when Dr Foulis first saw the patient, and that she was at the later period suffering from vegetative endocarditis.

Mr Cathcart said, with reference to Dr Gibson's argument that the blood-current could not pass from the pulmonary artery into the aorta, it must be remembered that it did so in the foetus, and that in this case they had a foetal condition existing to adult life. The flow of blood through the ductus arteriosus was just the persistence of part of the foetal circulation.

Dr George Balfour pointed out that the thrill and loud systolic murmur were propagated down the aorta. Theoretically, there should be no passage, but practically, he believed there was, and that it was probably aided to some extent by suction from the passing arterial blood.

Prof. Stewart further pointed out that the dilatation on the aorta, opposite the ductus arteriosus, was in favour of the view that the blood did pass from pulmonary artery to aorta.

Dr Foulis said he had to thank Mr Cathcart for using the very argument he meant to have brought against Dr Gibson's contention. In the foetus the duct remained patent till the first breath. It then began to close, and the best observers stated that the process of closure was usually over in ten days. But there was no reason why the foetal condition should not continue into adult life, and, as Prof. Stewart had remarked, the dilatation of the aorta opposite the duct showed that the ventricular systolic current had impinged on the wall of the aorta at that spot. There was no other way of explaining this dilatation of the aorta. With regard to Prof. Stewart's remarks about the presence of ulcerative endocarditis, what struck him at the time was this, that the extremely hard nature and fibrous character of the vegetations on the pulmonary and other valves of the heart at once showed that they could not have been produced in weeks, but had probably been forming during the course of years. In regard to Dr Wyllie's remark, the tricuspid valve was so competent that he could not see how it could assist in the production of the second bruit. In conclusion, he thanked the Society for the kind manner in which his paper had been received.

2. ON SOME POINTS OF IMPORTANCE IN MEDICAL PRACTICE.¹

By R. H. GUNNING, M.A., M.D., F.R.S.E., Ex-Pres. Roy. Physical Society, Ex-Lecturer on Anatomy, Dignitary of the Empire of Brazil, etc.

IN bringing the following remarks and cases before the Medico-Chirurgical Society, I believe they will be found to contain suggestions of practical importance. I mean to show how much can be done by hygiene and simple means, without the evils and expense of drugs and operations. I have long preached them abroad in my intercourse with members of the profession, but now submitted, more formally, to the appreciation of this practical and intelligent Society, I hope they will be duly studied, and that they will help to alleviate humanity with less suffering and at less expense. Our profession is not a mere trade to live by, but a priesthood for "the ills that flesh is heir to," and each of us must do his best, as he shall answer, to fulfil this mission.

I propose to-night to record and illustrate my experience in medical practice of bloodletting in pneumonia; of tartar emetic in strangulated hernia; of the horizontal position; of hot water; of regulated diet; and of the bad effects of over-drugging.

1. *Bloodletting in Pneumonia.*

The question of bloodletting in pneumonia is of great importance, from the frequency and danger of the disease. The treatment has gone from one extreme to another; but not for that have we attained to certainty. "In medio tutissimus ibis" is still, I believe, the best rule. In the question I have no prejudices, and I am quite ignorant of the polemic which has been waged on the subject since I went abroad, having been so long and so far away from books and periodicals, and having no love of mere polemic. I agree that a great addition of facts has been made of late years to medical knowledge by special or subdivided studies; but I do not admit that careful reasoning and the power of correlating facts have kept pace with and proportion to this great richness of facts. Routine is still obstinate, and guessing and mere experiment are too common, while cautious inductive reasoning is too rare. Pre-

¹ The topics of this paper were hurriedly written and joined together to avail of the monthly meeting of the Medico-Chirurgical Society, during my visit at the Tercentenary Celebration of the University. The short time allowed for the reading of so many subjects obliged me to be spare in argument and illustration, leaving them to be brought out in the discussion subsequent to the reading. This explanation must apologize for the meagreness of my remarks on Hot Water, Regulated Diet, and Abuse of Medicine, each of which might occupy a whole paper. I hope, however, that, as meant, they will be *suggestive* in practice.

ming these remarks, I will submit my own experience in the treatment of pneumonia to the judgment of the Society.

In 1849, I went to Brazil as medical officer to a large gold-mining company to recover health in the fine climate of that part of the province. It was my intention to return to Edinburgh to follow my career in anatomy and physiology, but the benefit of the climate, and success in a quiet practice, induced me to stay on and on for much longer, till, from one cause and another, I was 33 years in the country.

I had a hospital at a large gold-mining company for natives and Europeans, besides private practice at agricultural estates, in different directions, in a wide circle. The labourers on these estates were daily exposed to sun and rain, and easily took bronchitis, pleurisy, or pneumonia. In the gold-mine they were much exposed to cold currents of air and wet while much heated with the heavy work of blasting, transporting, and breaking up the masses of stone containing the gold. The proprietors of agricultural estates are all great doctors, or "curiosi," and only send for regular doctors when they themselves despair of success. With this delay, I found cases of pneumonia recovering when I arrived; and remembering Dr Alison's teaching of cures effected without bloodletting, I let the patients alone, and merely advised a blister, placebos, quiet in bed, and care in diet. In this way they recovered, and my lancets were in disuse for nearly 10 years, and I felt flattered that I had many cases to support the new non-bleeding school. The gold mine not succeeding, the labourers were brought down from the interior towards the metropolis, Rio de Janeiro, to assist in making the first great national carriage road. Besides them were, say two to three thousand others (blacks, Brazilians, Portuguese, Italians, and Germans), employed in heavy earth-cuttings, blasting of rock in and out of tunnels, and at building bridges over rivers, etc. This new locality, where I also had a hospital, furnished me with many cases of pneumonia, and I continued the same non-bleeding treatment. In the hot season, those sequelæ of pneumonia—dysentery, diarrhœa, hæmorrhoids, swelled feet, and liver complaints—show more markedly than in the cold season, and thus much labour is lost. On this account my attention was drawn by Brazilian professional brethren and owners of slaves to the greater frequency and more marked character of these secondary symptoms in cases treated by me than in those treated by others by bloodletting. I could not deny the truth of the criticism, and took it to heart.

I changed my plan, from conviction that I was in the wrong, and also to satisfy the owners of the slaves. The rationale of the use of bloodletting, as taught by Dr Alison, came clearer back to me, and all I have observed for many years since, in numerous cases, convinces me of its superiority.

First of all, I had the patient bled in the arm, to diminish the

tension of the circulation and relieve the distress in the chest, and in the standing posture, to favour, by faintness or nervous collapse, the arrest of inflammation and fever. I usually took from 4 to 8 ounces, and the patients would implore me "for God's sake" to bleed them, and as soon as done would add, "thank God" for the great relief. Along with this, I had a large blister put over the inflamed part, and the bowels were cleared with tartarized saline solution if strong, or with castor oil if delicate by age or constitution. Food was kept away till a favourable reaction set in and appetite was manifested, and when given was in small quantities, and of mild quality. Bloodletting, evacuation of the intestines, a blister, abstinence from food, and quiet, were the effective and regular treatment, but if the skin required to be kept moist, the *smallest quantity* of tartar emetic in solution, as a *diaphoretic only* could be used, as could be an opiate for sleeplessness or unusual pain.

The blister is a good part of the cure. First, it protects the inflamed part; next, by breach of surface and serous exudation, it relieves the circulation in the inflamed lung. It is a kind of second bleeding, like the watery discharge from the intestines by a saline purgative. Both discharges keep down the tension in the circulation without wasting the hæmatozine, and both can be kept up or dispensed with according to symptoms. An empty state of the digestive tube also helps much to check the inflammation and to favour reabsorption. Men and animals starved or semi-starved soon lose weight by absorption of normal tissues. In like manner abnormal exudations or tissues are reabsorbed, and more easily so, than normal tissues by an empty or starved state of the intestines. But neither this nor the blister nor saline depletives should let us dispense with bleeding at the arm as soon as we see the patient.

The rationale of bloodletting is well given by the late Dr Alison. First, the tension in the chest is relieved by the opening of a vein, on simple hydrodynamic principles. Then the check and reabsorption of exudation are favoured *endosmosically* on Dutrochet's principle, by lessening the volume of the blood, and by the serous depletion from the skin and intestines, caused by the blister and saline purgative. In proportion as the area of the inflammation is limited, and reabsorption complete, will be lessened the secondary complications of enlargement of the liver, dysentery, anasarca, etc.

I never repeated the small bleeding. The exudation from the blistered surface and the discharge from the bowels were enough, after the bleeding, with abstinence from food and perfect quiet, to promote curative reaction.

In all these cases, whether pneumonia proper or mixed up with bronchial or pleuritic complications, I only lost one in the acute stage,—a young person treated outside the hospital at a time of ex-

cessive heat, and where the relatives had given first their own nostrums. Of course the genial climate of the table-land of Brazil was a great advantage over the climate of Great Britain; and then the patients were all comparatively healthy working men of simple habits.

I lost several, however, from six months to two years afterwards in the non-bled cases from the sequelæ,—from enlarged liver, and ascites, or dysentery, etc. They were cases of slaves whose masters would put them back to heavy work, only to light up the old inflammation and its consequent sequelæ from central obstruction.

The conclusion, however, which I wish to inculcate is, that the cases bled only once, and in so slight a manner, were cured *more immediately* and *more completely* than by my previous practice of leaving them alone. I at once relieved the dyspnea and arrested inflammation, and the sequelæ ceased which so often followed the non-bleeding practice. In short, I would not like to ignore so simple and innocent a practice as the best preliminary to a blister, and evacuation of the bowels, and strict dieting. Let the unbiassed try the two plans in similar circumstances to judge which soonest and best relieves the sufferer. Enough that I record my experience.

2. *Tartar Emetic in Strangulated Hernia.*

One day, 25 years ago, a patient was brought to the hospital in a moribund condition, and vomiting stercoraceous matter. Not being able to reduce the hernia, and knowing the heavy voluminous nature of the man's food, I gave him two grains of tartar emetic to empty thoroughly the bowels. The effect was wonderful. He vomited much solid matter mixed with gases, and, with the abdomen thus empty, I found I could reduce the hernia. The case impressed me. I reasoned that such gases and solid matters very often are the true hindrance to reduction, and that the first thing is to empty the bowels well, and *make space in the abdomen* for the return of the intestines strangulated and extruded from the cavity. I often afterwards tried the same plan with like success, and all I wish under this head is to advise the practitioner to try this plan, when unable to reduce the hernia, before operating. It is a great matter to save the patient pain and risk. I use tartar emetic merely as the simplest and quickest emetic.

It might be feared that vomiting would aggravate the strangulation, but it is not so. Perhaps the vomiting moves a little the strangulated portion, but in any case it clears out all gases and solid matters, and makes the abdominal cavity empty enough to receive back the extruded intestines.

The following case, which occurred before I left Rio, is pertinent to the above:—A distinguished European contractor found an old hernia irreducible, and was in great suffering. The doctor attending him called in a naval surgeon and a distinguished professor of

surgery for consultation. Unable to reduce the hernia, they resolved to delay operating till some hours later on; but before the hour marked arrived, the patient had belched up gases and solid matters, and was able himself to reduce the protruded bowels. He was a nervous gentleman, and dreaded an operation; and of course the surgeons in conference were glad not to have to operate. The case, however, shows how important it is to have the bowels clear of their contents for successful manipulation.

In conclusion, it is an easy experiment; and if still unable to reduce the hernia, the necessary operation will be all the easier and safer from such a complete evacuation of the intestines.

3. *Horizontal Position.*

It is enough to look at the veins on the back of the hand or inside of the leg, to see the effects of hydrostatic pressure. The limbs being perpendicular, the veins swell; placed horizontally, they become again normal. If so in the limbs where the veins have valves, more so in the veins where there are no valves, as in the lower intestine and in the reproductive parts. How easy to prevent varix, varicocele, piles, and leucorrhœa, by reclining sufficiently, or to develop them by overstanding or overwalking. This is what I think is not sufficiently estimated in books, nor in practice. Too much is expected from local applications, or operations of one kind or another, and too little is trusted to the help of position, or physical law.

Then we must not forget that the force of the heart and general circulation is also diminished by the recumbent position. The pulse increases in frequency by sitting up, and more by standing.

4. *Hot Water.*

Hot water is recognised in many cases and in many ways as a good therapeutic agent, but I do not think the recognition is so general as it should be. Wherever pain or inflammation exists, it is a most powerful calmative. Opium and other narcotics remove pain by deadening the nerve-centres; but hot water seems to act by direct dynamic action on the local nerves, and by relaxation of local tension. However this may be, it gives great and immediate relief wherever pain and inflammation co-exist, as in acute ophthalmia, whitlow, sprains, burns, piles, cystitis, inflammation of the rectum and reproductive organs, painful menstruation, and acute leucorrhœa. It must not be merely tepid, but *as hot as can be borne*, and it must not be for a short time, but for as long as the pain continues. Fomentations and poultices, when medicated, mean more as heat and softening than as medicines, though these get the chief credit. In whitlow, it will prevent, if early used, but in any stage it eases pain, and helps suppuration as nothing else can do should the bone necrose. On one occasion a

heavy iron fell on my large toe. It took immersion in the hottest water I could bear for a whole night to settle the terrible pain and gain sleep, but from thenceforward I was at ease though I lost the nail gradually. Losing the nail showed how severe the bruise had been.

Ladies suffering agonies in menstruation are better relieved and cured by very hot hemicupia than by anything else. Of course, complications must be attended to at the same time. The bowels may need attention, or a weak circulation and cold extremities may need a hot stimulant—hot punch—but such complications being attended to, the hottest hip-bath is the surest means of relieving pain. And not only in painful menstruation, but when defective or in excess. Used for successive periods, the first is increased and the second is diminished. In acute leucorrhœa or irritated rectum, in the tenesmus of diarrhœa, besides the hip-bath, hot water—simple, or medicated with opium—can be injected into the passages. I even use a very warm hip-bath after labour when the lochia are excessive and the patient is feverish. In this way ablution is effective, the parts are soothed, and a source of puerperal fever—especially in warm climates—is removed. I have the patient carefully lifted and set down in a plentiful and very hot hip-bath. A recent illustration of the use of hot water occurred to me in Italy. A lady's absence from the table d'hôte was daily regretted by her husband and friends, and medicines were doing her little good. I found she had had an abortion about a month back, and that the hectic and lassitude were due to painful post-partum discharge. I advised her to sit down at once, and again and again, in very hot water, and also to inject it plentifully. She was soon relieved, and on the second day was at table, and out everywhere quite well. She might have got emaciated, hysterical, and ruined in health, ere the medicines of her routine doctor would have cured her.

While on hot water and the hip-bath, let me mention what has occurred to me as of wide social importance. It is the use of baths for the poor, especially for women. The want of ablution for the whole body, but especially for the digestive and reproductive outlets, is a cause of much discomfort and many diseases, especially in women. Public baths are too expensive and not at hand, and have never been availed of, except by a very limited number of the community. What then is best? It is simply for medical men to make a propaganda of good, cheap hip-baths to families. The first cost is within the means of the poor, and would save much suffering and much doctoring. Every day it would be ready for cold water, while the tea kettle could be enough to compose a good hot-bath when needed.

5. *Regulated Diet.*

The function which influences all others most is digestion. This all right, all is right, and *vice versa*. Cures, after operations,

are accomplished by nutrition, and this will be good, or the contrary, as the digestion is good or the contrary. This subject of digestion and nutrition should be well understood by surgeons. The preparing a patient for an operation, or treating him after, hangs much on this. Whatever dexterity a surgeon may have he should also understand physiology well to have success. He may fail sometimes from neglect of the simplest dogmas of physiology. A friend and myself, when abroad, had many cases of cataract. We did the operation in the same way; he better than I, for I had no gift of hands. He lost very many of his cases, and mine succeeded. He said, one day, he could not understand this. I replied, "It is simple; you let your patients *eat and drink* as they and friends like after the operation, while I starve mine for one or two days till the wound is healed and the retina is safe." A full stomach or improper food will soon befool the best done operation. I have cured serious fractures without fever intervening merely by semi-starving the patient. In my own case, when shot in the head, I abstained from all but water, or a little tea or dry toast, for days, and had scarcely pain and no fever or erysipelas. After days I used stewed prunes, potatoes mashed with salt butter, to be simple and laxative. I took no wine and no meat for weeks, and a remarkable cure was the result.

In all cases, medical or surgical, attention must be given to the digestive organs, to see that no peccant or irritating matter exists, and that they be carefully watched afterwards.

I once treated a lad for fracture of both thigh bones, a bullock-car having passed over his limbs. I set the limbs, and kept him semi-starved for days. Only gruel or weak chicken broth was allowed. He had no fever and got round beautifully. On another occasion the car passed over him and killed him. In short, let all ingesta be in the smallest quantity, and of mild quality, to avoid inflammation. Spirits in every shape are injurious.

6. *Abuse of Medicines.*

I believe strongly in the proper use of medicines, as anodynes, emetics, purgatives, diaphoretics, diuretics, etc., but I am also sure of the much abuse of them. In 1834, while attending a provincial infirmary, I was struck with the recovery of patients given up to die. The medicines, especially mercury, iodine, and iodides, would be suspended, and the nurse have orders to treat the patient kindly before dying. Immediately recovery would occur! This impressed me, and ever since in practice I have studied whether medicines were doing good or harm. I always held, and still hold, that homeopathy is chiefly the non-abuse of medicines. In the time of Dr Black and Professor Henderson of this city, when this question was once publicly discussed, I challenged its advocates to try 100 cases without medicine and

100 cases treated *secundum artem* against 100 treated homœopathically. I affirmed that, in the first case, the result would be as good as in their 100 cases, and that in the second mode there would be more success. The challenge was not taken up.

In Brazil my success was in cases treated by others. I merely suspended medicines, and put the patient upon hygiene, science, and common sense, correcting ignorance, neglect, and bad habits as to food, air, exercise, baths, etc. My success in chronic cases was great, and people declared that I must have used medicines, and very strong ones too, for I could not have made such cures otherwise! Some selected cases, further on, will be examples of this abuse of medicine, of diet, and of the horizontal position.

Emetics, purgatives, anodynes, etc., relieve symptoms but do not go to the root of the matter—error in diet, dress, habits, ventilation, etc. As symptoms recur they have to be repeated. This was the English system of “dose and cure” of the glutton or the beer-swigger, only to “dose and cure” him again. The pills, potions, and powders were the general practitioner’s chief income, more than from *honorariums*. But homœopathy, with its metaphysics and paradoxical wonders, and hydropathy, with its hygiene and appropriated science, have done much to correct this abuse of drugs.

Mercury is a good medicine in its place and in careful quantity, as is iodide of potassium. They will destroy abnormal exudations or organization, and the germs of syphilitic taint, but carried beyond this object they will debilitate and destroy the body itself. Quinine is a good tonic and specific, but in large doses it is most pernicious. And why in big doses, if small ones will serve? What deafness and loss of sight has it not caused? I have known a child made deaf and dumb with one large dose. I have seen delicate patients take 50 grains in a day and succumb, whose stamina were not equal to 10 grains. Small repeated doses can do all, I believe, that large doses do, without doing the mischief.

At first when yellow fever appeared in Rio, the deaths were few when an emetic or castor oil were given early, and the case was left to careful nursing. Since then, and now, a large part of the mortality is from so-called scientific or guessing and heroic treatment.

So with chloral and chloroform,—both good agents when carefully used. I know little of chloral in practice, but I have studied chloroform, and I believe few accidents would occur if it were well understood. When Sir J. Simpson introduced it into practice I made many experiments on dogs and rabbits, and was convinced that it acts directly on the capillary circulation in the lungs, and that death occurs after the manner of drowning when taken too long or too much. If this is so, then it is all-important not to delay its operation too long. A person under water so many

minutes may recover, but when so many minutes more it is fatal. The assistant who gives chloroform should understand this, and be most watchful. He should note the quantity used and the time, and it should only be given *on the operating table*, and when all is ready for operating. Thus the time is shortened all possible—the great point. It should not be applied and withdrawn at first, but allowed to have its effect at once. Inattention to this wastes much chloroform, lets the air disengage the circulation in the lungs, reawakening the patient, and saturates too much the body. A good simple plan is the piece of surgeon's lint laid over the nostrils and mouth, and wetted sometimes if needed. If overdone, fresh air and artificial breathing are indicated, the start for reaction being in the lungs, not in the heart. I could easily restore dogs apparently dead by tracheotomy.

I will now conclude by mentioning some cases which will show the influence of these four circumstances, namely, diet, position, hot water, and suppression of drugs.

1. A young slave woman had a chronic ulcer on the shin, of seven years' standing. Various doctors had been consulted. Iodide of potassium and calomel were used in succession till most of her teeth were lost, in the belief that she had a syphilitic taint. Caustics and ointments were used externally. The ulcer looked hardened and desperate, and I concluded that, besides the irritating effect of the said medicines, externally and internally, the limb had not had due rest. I simply suspended the medicines, ordered a linseed poultice to keep soft the ulcer, and had her strictly confined to bed. In six weeks, more or less, her health was better, and the ulcer was cicatrized. Here the health was improved, no doubt from suppression of medicines, but the cure of the ulcer was mainly due to diminished congestion. In short, she had been previously treated without due attention to position, etc.

2. Next, a gentleman consulted me about a large ulcer on the anterior inner part of the thigh, of thirteen years' standing. The whole limb was swelled and anasarcaous. He had been the round of the doctors within a large circle. I thought the ulcer had been treated without due attention to position, and declared I would not treat him without a solemn promise that he would keep absolutely in bed. I started him with a dose of comp. powder of jalap to assist the riddance of the anasarca, and to convince him that I was using medicine, for medicine they must have. And, without more than a linseed poultice, diminished diet, and horizontal position, he was well in a couple of months or so. I saw him ten years afterwards quite well.

3. A gentleman patient suffered from an obstinate stricture of the urethra, of some years' standing. He would not allow cutting of any kind, and the surgeons in the city who had attended him were unable to pass any sound. The urine was

passed by several openings under and about the scrotum, which was ulcerated, and nearly the size of a man's head. He was ever up and on his feet, washing, and passing urine, and I saw a good chance of bettering his state by removing all this source of congestion and exudation. I ordered him, several times a day, a very hot hip-bath to soothe the parts (for nothing soothes like hot water), and then had him put to bed, with strict injunctions not to rise except for real necessity. I made him cross his legs, not to let the large tumour depend, and dressed the raw surface with simple ointment. I also lowered his diet, to facilitate absorption of the exuded matter, and allowed him to drink much rice-water to produce mild urine, as salted, strong urine irritates the rawness of the urethra and fistulous openings. In a week or so the swelling was down, as by a charm, the fever gone, and the parts again almost natural. I found I could get my smallest sound into the stricture. In the course of a day or two more I got a very small bulb-pointed one (sent me by Prof. Spence) through. By-and-by I passed other sizes, up to the largest, when the fistulous openings closed, and a complete cure was effected. This was done by keeping him merely horizontal, and the tumour elevated, not dependent. The soothing hip-bath, the reduced diet, and the diluted urine all helped, but the main thing was position, neglected by others more dexterous in manipulation than myself, but less cunning about the importance of position, hot water, and lowered diet.

This same gentleman returned, in a similar condition, after more than ten years, declaring that the surgeons of his city could not pass the sound to dilate, and that he could not submit to any incision. I treated as before; he returned home in two months or so, again cured.

4. I will give another case. A vicar, looked upon as a saint far and near, had a pimple on the upper part of the penis, behind the gland. His brother-in-law, licensed in Italy, and another, a graduate of Paris, treated him. Caustic was applied, but only inflamed the part more. Then they assumed that it had a syphilitic taint, and gave him mercury till the gums and all his bones were sore. The caustic was continued, and resulted in a subcutaneous fistula. Into this yellow wash was injected, and sloughs came away. The more sloughing the more injections, till the fistula reached under the pubic arch. The member got immensely enlarged, and also the glands of the groins. Then it was called cancer, and no hope of a cure could be given by them. The poor vicar made his will, and took the last sacrament of the church. Considered a very saint in the province, the lamentation was great at the prospect of losing him. An English doctor from a distance was called in. I had ceased to go from home to see cases, being now the director of a gold mine. Going to dine with a large landed proprietor, and meeting my English colleague, I asked for the patient. He said he could only think of amputation, but that

he did not mean to operate on the vicar. When at dinner a note came to me from a devoted friend of the holy priest, wishing me to see him, and not let him die. I said I could not go so far, having duties at home. My host said I must go, as the vicar was his cousin. Horses were got ready, and we galloped over the Campo twenty miles by moonlight, in the cold time of the year.

When I saw the sick man, I thought him poisoned with mercury and iodide of potassium internally, and externally by yellow wash. I did away with all, had warm flannels got, and a soft mattress to keep him warm instead of his hard bed and charcoal fire, filling the room with carbonic acid. I gave him a very warm bath with my own hands. With this comfort, and that of warm clothes, and a soft bed, and the suppression of irritating drugs, he slept, for the first time, for long. I injected warm water instead of yellow wash. The sloughing ceased, and the fistula closed, and the swelling everywhere diminished. In less than two months he was saying mass in the parish church, and the cure was regarded as a miracle! The family sent me a large fee, which I returned; for I never charged priests. The parish wished to seek a decoration from the emperor, which I declined; and, baffled in these things, the good folks put up a saint of my name in the parish church, in my honour, in spite of my being notoriously a great heretic!

A large part of this cure was the suppression of irritating, poisoning drugs; but the hot water, and strict lying position, were all great factors in the result.

These are illustrations of what position, the non-use of drugs, and the use of warm water and diet do in medical cases. Let me give a recent case, if not of the abuse of medicine, at least of its non-usefulness.

All know how long the greatest statesman of the age has suffered from gastritis and torturing gastrodynia. The routine of mere scientific prescriptions could do nothing for him. At length, in the hands of Dr Schweningen of Munich, he has been fortunate. This gentleman has discerned the case better, and enforced strict diet and a general change of regimen, and the great Chancellor is again well and at work.

In conclusion, more attention should be given to hygiene, and less trust put in drugs.

The President, in the name of the Society, cordially thanked Dr Gunning for his paper.

Owing to the lateness of the hour there was no discussion.

Meeting VIII.—June 4, 1884.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

I. ELECTION OF NEW MEMBER.

Frederick Anastasius Saunders, L.R.C.P. & S.E., Crail, was elected an ordinary member of the Society.

II. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Mr F. Caird* showed a KNEE JOINT in section, on which, about a year previously, Mr Chiené's operation for knock-knee had been performed. Amputation was necessitated on account of gelatinous degeneration. The preparation demonstrated that in the operation for knock-knee the joint had not been opened into, that perfect healing had resulted, and that the epiphyseal line occupied a normal plane.

2. *The President* showed (a.) THE PARTS FROM A CASE OF CUT-THROAT. It happened in the person of a constable who had been troubled for about six months with insomnia. One morning he got up about four after a restless night. His wife, who was sleeping in another apartment, heard a heavy fall and a deep moan, and going in found her husband lying on the floor and blood flowing from the neck. Strange to say he was able to articulate, and said he had cut his throat. His larynx was opened into, the epiglottis cut across. No important vessel was wounded, but there must have been considerable hæmorrhage. He was taken to the Infirmary, but died on the way. The body was taken home after two days, when the widow consented to have a post-mortem performed, the reason for this being that the man was insured, and he (Dr Littlejohn) was of opinion that if an autopsy revealed cerebral disease, the medical attendant was entitled to give such a certificate of death as would cause even those insurance companies who forfeited the policies of suicides to pay the policy. In this instance he was glad to say he found traces of chronic cerebral disease, great adhesions of dura mater to bone, adhesions in arachnoid cavity, a large amount of sub-arachnoid fluid, and roughness of the internal surface of the cranium, justifying him in giving a certificate of chronic cerebral disease with a wound in the neck, which was available in procuring the insurance money.

(b.) THE LIVER AND HEART, from a case illustrating the effects of a fall from a height. A heavy woman, aged about 50, threw herself from a window in Allan's Close. The distance to the ground was measured and found to be 80 feet. She sustained injuries to her head, compound fracture of one of the thighs. The liver had sustained rupture. The appearance of the heart was interesting. The base, as not unfrequently happened, was

ruptured, but in addition to that there were two very marked lesions, one in the right the other in the left ventricle. These were made out to be caused by two ribs which had been fractured and driven in upon the heart.

III. ORIGINAL COMMUNICATIONS.

1. TONSILLAR DISEASE IN CHILDREN.

By JAMES CARMICHAEL, M.D., Physician to the Royal Edinburgh Hospital for Sick Children.

THE frequent liability of children to affections of the tonsils is universally recognised. Disease of these glands is too often considered of minor importance, yet if we carefully watch the sequence of events in many cases, such conditions are apt seriously to affect the general health, and often form the starting-point of other and more serious ailments. There can be no doubt that we are sometimes apt to underestimate the importance, if not at times overlook, the slighter forms of tonsillar disease in young children. Nothing alarms such patients more than an examination of the throat, and we are apt to be deterred from pursuing our investigations in this direction if there be no special symptoms leading us to suspect the presence of disease. In no class of cases is a thorough and complete examination of the patient more necessary than in children, and, in carrying this out, we should never neglect to look carefully at the fauces. Tonsillar diseases may be conveniently classified into acute or chronic varieties, and these in their turn may be primary or secondary. The accompanying table shows in a nosological form the various affections of the tonsils to which children are liable, and may assist us in our consideration of the subject.

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| I. <i>Primary.</i> | { | <i>Simple Inflammatory.</i> —1. Simple or superficial catarrh. |
| | | 2. Follicular catarrh. 3. Parenchymatous tonsillitis, including tonsillar abscess. |
| | { | <i>Complicated.</i> —1. Croupous. 6. Syphilitic. |
| | | 2. Diphtheritic. 7. Tubercular. |
| | | 3. Ulcerous. 8. Irritant or toxic. |
| | | 4. Aphthous. (Ingesta poisons.) |
| | | 5. Parasitic. |

II. *Secondary or Symptomatic.*—Accompanying acute exanthemata and other diseases, such as scarlatina, variola, diphtheria, measles, typhus, pneumonia, dentition and dental caries, stomatitis, gastric catarrh.

III. Hypertrophy of tonsils.

I purpose in this paper to allude to a few of the more important of these varieties. In studying the diseased conditions of the tonsils it is necessary to remember their anatomical relations and structure. Lying between the pillars of the fauces on each side, they are barely visible in health when examining the throat. On the free surface of each gland are from ten to sixteen round or linear openings, leading into lacunæ or sinuses, which are lined with epithelial covering similar to that on the external surface. The gland substance consists essentially of reticulated connective tissue and lymph follicles, and is plentifully supplied with capillary bloodvessels, as well as lymphatics. These lymphatic vessels, embedded in the inter-follicular tissue, are very numerous, and communicate with several glands, generally termed tonsillar, lying at the angle of the jaw. In all affections of the tonsils they are liable to enlargement, and irritation may be set up in the other cervical glands.

In delicate or strumous children particularly, the glands are apt to become hyperplastic, and remain chronically enlarged, sometimes giving rise to abscesses. If inflammation does not occur, the glands may become caseous or tubercular, and the child then runs all the risks of general infection. In children it is now commonly admitted that you seldom have long-continued irritation of any mucous surface without implication, to a greater or less extent, of the lymphatic glands connected with the vessels supplying the part. We know in bronchial affections this is not unfrequent, likewise in gastro-intestinal disorders.

The simplest form of disease of the tonsils is *Superficial Catarrh*, characterized by slight redness of the surface of the glands, accompanied by increased secretion. This may be all that is noticed, no further development of the disease taking place. When, however, the catarrhal condition becomes more severe, redness of the surface is not only present, but also swelling of the mucous and submucous tissue, with extension of the catarrhal process into the follicles, this latter development being characterized by the appearance of small white points of secretion at the lacunar openings, denoting the true *follicular tonsillitis*, a disease which is met with in varying degrees of severity, accompanied by a corresponding amount of constitutional disturbance. The condition is readily recognised from the characteristic appearance of the tonsil. A complication is frequently met with in severe cases, more particularly in the symptomatic forms, characterized by an excessive amount of follicular secretion, accompanied by more or less croupous exudation on the surface. The spots of secretion at the openings of the follicles appear to become confluent from croupous exudation thrown out on the surface of the tonsil, giving rise to yellowish-white patches, often covering large portions of the surface of the gland. This condition is interesting from a diagnostic point of view, as in the early stages it is apt, and, I believe, sometimes is, confounded with diph-

theritic disease. The differential diagnosis can be made by a consideration of the constitutional condition of the patient, as well as the local appearances. Clinically we find in croupous tonsillitis the gland is of a pale red colour, and the patch yellowish-white, confined to the tonsil, and easily separable, leaving the mucous membrane denuded of its epithelial covering, but otherwise intact. In examining the exudation in these cases microscopically, in addition to the cellular elements of the follicular catarrhal exudation, we find blood corpuscles embedded along with the other cells in a fibrinous matrix. In diphtheria, on the other hand, the gland as well as the whole of the fauces is of a dark angry red tint, the patch is of a grayish or grayish-white colour, and cannot be scraped off without leaving a bleeding surface. The disease is not necessarily confined to the tonsil, but may extend to the whole of the fauces, and into the posterior nares, or over the epiglottis and into the larynx and trachea.

Zeigler¹ describes the *croupous exudation* "as a pale yellowish membrane, consisting of fibrinous filaments and granules beset with pus corpuscles," the cellular elements embedded in the matrix undergoing what he calls "coagulative necrosis." This membrane is very loosely adherent to the surface, and can be readily scraped off. The essence of the process appears to consist in abundant extravasation of liquid and cellular material, and "the absence of such agencies as hinder coagulation." The *diphtheritic process* he describes as one in which the "epithelium is not shed, but dies without desquamation, abundant liquid being at the same time poured out, which fills the cells, and gives rise to a condition of rigidity akin to coagulation." The exudation is rich in albumen. He describes a superficial variety of the disease in which the changes only involve the mucous surface proper, and says that what he calls this superficial diphtheria and the croupous exudations present many similarities, and are apt to be confounded together. In *parenchymatous diphtheria* a much greater extent of tissue is involved—not merely the epithelial surface, but also the underlying connective tissue. In both cases you have necrosis of the affected tissues, and a line of cellular infiltration separating the dead from the living. Haemorrhages are not uncommon, and coagula in the lymphatics. These exudations are generally loaded with lower vegetable organisms. The pathological distinction between these croupous and diphtheritic affections of mucous surfaces is one of great interest, some observers believing in their identity, and others that each has, in the great majority of cases, its own well-defined pathological characteristics. Clinically I think we may, as a rule, make a distinction, although in exceptional instances it is difficult to draw a "hard and fast" line. Certain it is, that you have a simple form of patch confined to the tonsils alone, and clearing off within a few days or a week. On the other hand, you meet with cases

¹ *Pathological Anatomy*, p. 225.

where the patches are more adherent, and tend to spread over the adjacent mucous surfaces, the disease taking several weeks to run its course.

Professor Henoch¹ differentiates clinically between the simple forms of sore throat with exudation and the diphtheritic throat. He says "certain anginas present a similarity to diphtheria. On the second day of the disease round yellowish-white patches, the size of a pin's head or larger, appear upon the tonsil; occasionally they are isolated, but often confluent, so that the tonsil appears covered with an irregular whitish-yellow mass, which looks suspicious, but usually, he says, these patches leave no doubt of their benign nature, they are composed of purulent material secreted from the follicles, are loosely adherent, and easily detached. The yellowish colour of the patch is different from the gray or white colour of diphtheritic exudation. Cases occur not unfrequently in which judgment must be delayed for from twenty-four to thirty-six hours. Within this time simple catarrhal angina is either at a stand-still or subsiding, while diphtheria steadily increases in severity. I attach no importance to the fever and enlargement of the lymphatic glands, as they are common to both conditions. Microscopically you cannot arrive at a diagnosis, as bacteria are also found in both. Many cases of severe catarrhal angina are undoubtedly regarded as diphtheria by superficial observers, and this explains the fact that many physicians cure almost every case of diphtheria."

Parenchymatous Tonsillitis is much less frequently met with than the other affections, and so with *tonsillar abscess*, which in young children is extremely rare. Why this should be the case we are unable to explain.

Complicated Forms.—Of these I have already referred to the *croupous* variety, and have nothing further to add, except that under certain conditions, I believe this form of sore throat may take on diphtheritic action. Last year I had a little girl under my care in the Royal Hospital for Sick Children, suffering from scarlatina. She had well-marked follicular tonsillitis with croupous exudation from the commencement of the disease. By the tenth day the patches had cleared off the right tonsil entirely; on the left one there still remained a very little patch, about the size of a small pea. On the twelfth day this tonsil became more swollen, and of a dark red colour, and diphtheritic ulceration rapidly spread from it to the soft palate, uvula, and nares, and also to the other tonsil. The child had a prolonged and tedious attack of diphtheria. I have seen other cases of a similar nature, going to prove that diphtheria may be, so to speak, grafted upon an ordinary sore throat. This occurs most frequently, I believe, in scarlatina. Diphtheria is no doubt frequently met with in this disease at the commencement, or it

¹ *Diseases of Children*, p. 286.

may come on at a later period, being thus grafted, as it were, upon an ordinary catarrhal condition of the throat.

The *ulcerous* and *parasitic* forms of sore throat are commonly met with in connexion with similar affections of the mouth in children, and do not demand special note.

An important point in connexion with these catarrhal inflammations of the throat, and one which is peculiar both to them and diphtheritic affections, is infectiousness. It is a matter of common experience that, under certain conditions at all events, ordinary catarrhal sore throat is infectious. In a family you seldom have one case without others. So much so, that we constantly hear it said that the "sore throat has gone through the house." It seems probable that catarrhal diseases generally are more or less infectious. Conjunctival catarrh and bronchial catarrh are not unfrequently so. Some years ago, when a number of tourists visited the island of St Kilda, one of the party, in writing an account of the excursion, mentioned an interesting fact confirmatory of the infectiousness of ordinary catarrh. He stated, that the islanders were always afraid lest any of the visitors who came ashore were affected with cold, because it not unfrequently happened that the disease spread through the island. In the light of recent pathology, we find a possible explanation of the infectiousness of diseased conditions of mucous membranes, especially of the mouth and fauces. We know, that in children at all events, the buccal mucous surface forms a suitable nidus for various kinds of vegetable micro-organisms, the *Saccharomyces albicans* (Reess) or thrush fungus flourishes readily in the mouth, and so, in diseased conditions of the mucous surface generally, we may find micrococci and other vegetable forms. These, no doubt, in some way or other, may act as bearers of infection, taking root on any mucous surface, which forms a suitable nidus for their growth. An interesting point, and one well deserving of future investigation in regard to the simpler inflammatory affections of the tonsils, is to what extent these glands may form a nidus or a point of entrance for pathogenous micro-organisms into the lymphatic system. The question, for instance, whether the bacillus tuberculosis in this way gains entrance into the lymph spaces, with which these glands abound, is worthy of attention. The extreme liability of children to tubercular infection is well known, and in them it is often difficult to determine the starting-point of the disease. The clinical fact, that diseased conditions of the buccal mucous membrane, whether catarrhal or otherwise, are usually infectious, is, I think, sufficiently proved.

Symptomatic or secondary forms of sore throat are met with in the eruptive fevers, more particularly scarlatina and measles.

The *scarlatinal form* of tonsillitis is one of the most interesting and important met with in children. It occurs in all degrees of severity. The simplest form is characterized by bilateral redness

of the surface of the tonsils, edge of soft palate, and uvula. This usually lasts a few days, and, in a healthy child, disappears along with the eruption, leaving no immediate bad effects. The tonsillar lymphatic glands may sometimes be felt slightly enlarged, generally in delicate or strumous children, in whom irritation is more easily set up. The next form of tonsillitis, in degree of severity, is essentially of a *follicular* nature, both glands, as well as uvula and adjoining soft palate, being red and swollen; the secretion is excessive and of a sticky, mucous nature, but not of a sanious or irritating character. It is in this variety of throat affection that we so frequently meet with yellowish-white patches on the surface of the glands. These are usually of a simple nature, and present the characters of the croupous exudation already described. The throat generally gets well in a week or ten days, but the tonsils are liable to remain more or less hypertrophied. Parenchymatous tonsillitis is, compared with the two forms just mentioned, rare in children under seven. Above that age it is met with more frequently. It may or may not end in abscess.

The *apthous* and *simple ulcerative* forms of tonsillitis are generally associated with similar conditions of the mucous membrane of the mouth, either during dentition, or as a complication of gastric catarrh, the result of improper feeding or defective hygienic conditions.

Diphtheritic throat is not an unfrequent complication of scarlatina. It may occur, as already stated, during the first week of the fever, or may come on at a later period, as secondary to a more simple form of tonsillar disease. Occasionally, cases of diphtheritic throat are met with, during scarlatinal epidemics, without any rash, a case of this kind perhaps occurring in a family where there are other cases of scarlatina. The question of the peculiar relation which appears to exist between scarlatina and diphtheritic affections is interesting. There can be no doubt that diphtheria frequently complicates scarlatina. In regard to the former class of cases, the question naturally presents itself, whether they are instances of primary diphtheria, or scarlatina without eruption complicated with diphtheritic tonsillitis. This, it appears to me, is very much the point at issue.

In certain epidemics we meet with cases of so-called diphtheria, perhaps, as I have already indicated, one case occurring in a family, several other members of which are actually suffering from scarlatina. My observation has led me to believe, although I do not feel inclined to give a dogmatic opinion on the point, that there are cases of scarlatina complicated with a malignant form of diphtheria, with little or no eruption. In certain epidemics, undoubtedly, we meet with cases of scarlatina maligna, with very scanty eruption, and these often complicated with diphtheria. Dr Morell Mackenzie, in treating of diphtheria, seeks to get over the difficulty by describing a cutaneous eruption in some cases of

diphtheria. He says the eruption is sparse, and not of a very definite nature, that it "more or less resembles the rash of scarlatina, but that it differs from it in not being followed by desquamation." Now, we know that desquamation takes place in scarlatina in direct proportion to the amount of eruption; if this be so, we would not expect desquamation in such cases as he describes, even on the supposition that they were of a scarlatinal nature, and I humbly submit that Dr Mackenzie has not proved the negative. The scarlatinal eruption, when scanty, is of so evanescent a nature that it is apt to be overlooked altogether, and it would appear probable that these cases of diphtheria, occurring in close association with scarlatina, may either be a malignant form of the disease without eruption, accompanied by diphtheria as a complication, or the eruption may be so sparse or evanescent, especially if the cases have not been under continuous and close observation from the first, as to escape detection altogether.

The other *symptomatic forms* of tonsillitis most commonly met with, in addition to the diphtheritic and scarlatinal, are in variola, measles, typhus, and also in croupous pneumonia.

In *measles*, the throat affection is usually of a simple catarrhal nature, and gives rise to little inconvenience at the time; afterwards, as a result of the inflammatory condition, the tonsils may remain more or less hypertrophied.

In *typhus*, the tonsillar affection is rarer than in measles, and usually of a simple inflammatory nature.

In *croupous pneumonia*, tonsillitis is sometimes met with in children, often usually associated with aphthous stomatitis, a similar condition of the mucous membrane to that of the skin of the lips, which so often shows a herpetic eruption.

During dentition tonsillar inflammation is seldom absent, when you have any degree of stomatitis. It may be of a simple catarrhal or follicular nature.

Dental caries.—In older children, who are shedding the milk teeth and cutting the permanent ones, you meet with tonsillar inflammation, apparently as a result of buccal irritation.

In *all the forms of stomatitis*, but more especially in the aphthous variety, you are apt to have tonsillitis as a complication.

In *the simpler forms of gastric catarrh*, you meet with a congestive form of tonsillar affection, in conjunction with superficial pharyngeal catarrh.

Hypertrophy of the tonsils is frequently met with in children, and the question which always seems to present itself, Is this hypertrophy ever of a primary nature, or is it the result of previous inflammatory affections of the glands? The decision of this question is not easy, on account of the difficulty of ascertaining in any given case of hypertrophy, which is brought to the surgeon, whether the child has been previously the subject of the milder forms of catarrhal or inflammatory affection of the glands. In young

children, as we know, the symptoms are objective; the child cannot tell whether, during some slight previous ailment, it has felt the throat sore or not. Children are liable to the milder forms of these diseases from cold, or in association with the process of dentition. Gastric catarrhal affections are more or less frequently accompanied by the milder forms of tonsillar disease, which is apt to be overlooked if a careful examination of the fauces has not been made. My own experience would lead me to believe, that in the great majority, if not in all cases of hypertrophied tonsils, the enlargement of the gland is secondary to repeated mild attacks of a catarrhal or inflammatory nature. It has been supposed that in delicate and strumous children, the tonsils and lymphatic glands may become enlarged from purely constitutional causes. I think this may, very reasonably, be doubted. It would seem more probable, that while such a constitutional condition as scrofula undoubtedly predisposes to disease of the glands, some exciting cause is required for its development.

Hypertrophy of the tonsils in children gives rise to a variety of conditions, in themselves more or less attended with distress and even danger. Dupuytren, Shaw, and others, have related cases where the development of hypertrophy at an early age induced, by obstructing the respiration, deformity of the chest or chicken breast very like that of rickets. Politzer¹ says narrowing of the nostrils is a frequent result, and when present may be considered a pathognomonic diagnostic sign of greatly enlarged tonsils. Cases are related where, in the absence of any other source of peripheral irritation, enlarged tonsils may produce laryngismus stridulus or spasmodic asthma, in the same way as we know nasal polypi give rise to reflex irritation.

Hypertrophied tonsils likewise predispose to repeated attacks of catarrh, which debilitate and annoy the child. Perhaps the most troublesome and serious result of tonsillar disease is cervical adenitis. The tonsillar glands become readily affected, and disease may spread to any of the other cervical glands. Far more satisfactory results in treatment may often be attained in the earlier periods, in these cases, by directing our attention to the cure of the tonsillar disease, than by local treatment of the glands, even if accompanied by the usual constitutional remedies. I cannot, however, in the scope of this short paper enter into the treatment of these affections. My object has rather been to insist on the importance of their early recognition with a view to treatment and the prevention of ulterior complications, which are so apt to develop themselves in children.

The President said they had listened with much pleasure to this exhaustive paper on those troublesome affections of children. He should like to ask Dr Carmichael whether he met with those conditions most frequently in his private practice or at his

¹ *Jahrbuch für Kinderheilkunde*, Band **xxi**, Heft 1 and 2.

cliniques in the Sick Children's Hospital; whether he had noticed that they were more prevalent on certain days of the week, Dr Alison having remarked that sore throats in children were more frequent on Mondays and the beginning of the week than at any other time, and attributed this to the effects of the usual Saturday night house-washing; and lastly, whether they occurred more frequently in one locality than in another? Canal Street, which was now swept away by the City improvements, was at one time very much affected.

Dr Rattray of Portobello said he had seen a good many cases of scarlatinal and diphtheritic sore throats, and was much struck with the difficulty that sometimes occurred in the differential diagnosis. This was more apt to happen at the beginning of the affection, for he thought that later on there should not be much difficulty. As to liability of certain localities, in Pipe Street, Portobello, they had a great many throat affections not sufficiently pronounced to enable them to say what the nature of the disease might be. Mothers often brought children to them without knowing that the throat was affected. No. 5 High Street was another evil locality, which he would like very much to have condemned by Dr Littlejohn.

Professor Chiene said he should like Dr Carmichael's opinion on one point. He had listened with much pleasure to the paper, but regretted that no allusion had been made to the treatment of hypertrophied tonsils. Excision of these was, he thought, very much overdone; but he should like to ask Dr Carmichael what he thought on the matter.

Dr McBride said he had been struck by one or two points in Dr Carmichael's paper. The first of these was the differential diagnosis between croupous and diphtheritic inflammation. Was it possible to make a diagnosis in severe cases of croupous tonsillitis? He did not think it was always possible, because almost all the points of difference as laid down in the text-books failed. The second point was that Dr Carmichael had spoken of certain forms of non-symptomatic tonsillitis as infectious. This was a new fact to him. Catarrhal affections of the mouth and nose were undoubtedly infectious, but he had not previously heard of catarrhal tonsillitis being so. He wished to ask if Dr Carmichael found that such was the case in his own practice. Thirdly, as regards Dr Carmichael's view that croupous inflammation sometimes passed into diphtheritic. Was it not rather that a croupous inflammation left the tonsil in a weak state, and liable to take on diphtheritic action when exposed to infection? In the fourth place, and in reference to remarks previously made, were not hypertrophied tonsils much abused organs? In many cases the mucous membrane of the nose and the pharyngeal tonsil were enlarged and obstructed respiration, but given hypertrophied tonsils alone, were they of themselves sufficient to obstruct nasal

respiration? He did not quite see how they could, unless enlarged so as to fill up the whole naso-pharyngeal space.

Mr A. G. Miller wished to join *Mr Chiene* in asking *Dr Carmichael* his opinion of excision for hypertrophied tonsils, and also his views as to the use of gargles in tonsillar affections. On the latter point, he himself was inclined to believe that gargles were singularly inefficacious. It did not appear that they reached the tonsils, and one seldom could get children to use them properly. As to excision, he thought with *Mr Chiene* that hypertrophied tonsils were too frequently meddled with. Some years ago, he was inclined to remove enlarged tonsils whenever he saw them, and once or twice in dealing with soft tonsils he was annoyed by the way in which the tonsils tore. He tried painting with some astringent, as tannic acid and glycerine, in order to harden the tonsils for removal, and found that this treatment was often sufficient without excision.

Dr James wished to refer to the backward pressure effect of enlarged tonsils on the chest, and the justifiability of their removal in certain cases. As to the bad effects, pigeon chest and so on, physiologically, it was easy to see how these would occur. They knew that, if they took the respiratory power in man, air passed in and out with, in inspiration, the slightest amount of negative pressure, and in expiration the slightest amount of positive pressure. They found, if they diminished in any way the size of the opening by which air passed into the chest, the fluctuations of the mercury in the manometer were greatly increased. When they remembered what that meant over the surface of the chest-wall, they could understand how the slight enlargement of the tonsils would produce this change in the growing chest-wall of the child. They should remember that very slight obstruction was sufficient to cause chest deformity. He had seen pigeon-breast brought on by enlarged tonsils, and disappear when the tonsils were removed. He had seen a similar effect produced by a nasal polypus, and also disappear after its removal. It must be remembered, as *Dr Carmichael* had pointed out, that hypertrophy of the tonsil was frequently associated with a small contracted condition of the nostrils. People with enlarged tonsils had always to breathe through the mouth. He, therefore, was in favour of operating on the tonsils where there was any chance of deformity of the chest occurring.

Dr Allan Jamieson had been, like previous speakers, very much interested in *Dr Carmichael's* paper. He wished, however, that a little more had been said about the etiology of these tonsillar affections in children. In one point he was interested, the association of scarlet fever with diphtheria. It was evident, as *Dr Carmichael* had brought out, that the diphtheria did not arise out of the scarlet fever, but that the diphtheria was superadded to it. Some years ago he had attended a family with scarlet fever in a

large house well drained. Each of the children after recovery developed diphtheria. When they inquired into the cause, they found that the drainage was conducted into a cesspool some distance down the hill from the house, which had not been cleaned out for a very long time, and the gases having ascended to the house, found the throats of the children in a weak condition, and produced the diphtheria. They all recovered. Another cause of tonsillitis seemed to him to be that in Edinburgh it was a custom for people going away for a time to shut up the house. The water, even in the best trap, was apt to dry up when the drains were left unused, and deleterious gases were apt to get into a well-drained house and contaminate the house and cistern, and so lead to diphtheria. In one instance a servant was sent home to clean the house before the return of the family. She got a sore throat, but said nothing about it. One child after another on going home took sore throat, it being infectious. It was not till the third or fourth child had it that she thought of telling, and so cleared up the mystery. Dr McBride had spoken of the occurrence of diphtheria upon tonsils due to the lowered conditions of the tonsils. He saw a curious instance of this a short time ago. A butler had a severe attack of diphtheria consequent on the defective drainage of his master's house. A year afterwards, when the family were away from home, one of the servants went to the Infirmary for advice regarding her tonsils, which were enlarged. She was advised to have them removed. At the time the tonsils were evidently in a soft state. When she returned home a diphtheritic deposit appeared on the cut surface of the tonsils. This lasted for a time with constitutional effects, albuminuria, etc., but the girl eventually got well.

Dr P. A. Young thought some of the points raised by Dr Carmichael of great practical importance, more particularly the differential diagnosis between croupous and diphtheritic inflammation. Almost one of the first cases he was called to after commencing practice was a case in which he had diagnosed croupous inflammation. It was attended with painful results, and made a great impression on him. He had been taught to believe that croupous inflammation was not infectious. He therefore took no precautions. A second member of the family took the affection and died with all the symptoms of diphtheria, though the first got well. He was sure that a bad case of croupous inflammation and diphtheria could not be differentiated. Croupous tonsillitis, he believed, was really a mild form of diphtheria. The cause of the affection might be helpful in enabling them to decide what to do. Was the cause a change in the temperature, or was it due to drainage? If the first, there was no necessity of making the patient's friends turn the house upside down. In the second case, the drains must, of course, be attended to. The real cause was not, however, always easily determined. He did not know whether Dr Carmichael had

noticed that when diphtheria was epidemic tonsillar affections came on, and very often after an attack there was a relapse. As to the infectiousness of coryza, he had seen lately a case very illustrative of this. A gentleman suffering from thoracic aneurism was put to bed. They were very anxious to prevent him sneezing. His little boy having a nasal catarrh was ordered to be kept apart from him, but on two separate occasions he managed to get beside his father, and in both instances infected him badly, and greatly risked his life. There had been no change in the temperature to cause such a thing as a cold in the head.

Dr Gunning said his experience was that these tonsillar affections were usually got rid of and prevented by the use of hot-water gargles and the wearing of flannel. He had been very subject to sore throat himself, but since he began to wear high collars he had not suffered.

Dr Carmichael, in replying, remarked that his paper was not meant to be exhaustive, and he had purposely avoided entering upon the etiology and treatment of these diseases. In answer to the President's questions, he could not say whether he had observed a larger proportion of these cases in private practice or at the Sick Children's Hospital. He had first begun to study them carefully in the out-patient department of the hospital, but he could not say that the proportion was greater there than in private practice. He had not noticed that they occurred on different days; but they did seem to prevail in certain localities, more particularly in low-lying and damp situations. As to the excision of enlarged tonsils in young children, he did not recommend it, unless they were interfering very much with the child's general health. By the use of local and constitutional means it was wonderful how they improved as the growth and development of the child proceeded. In reply to *Dr M'Bride*, in regard to the differential diagnosis between croup and diphtheria, he would remind him that in his paper he had said that in some cases it was impossible to draw a distinct line between these diseases; but as a broad clinical fact, he agreed with *Henoch*, that if the exudation did not soon begin to clear off, and the case improve, it was probably diphtheritic. His experience was that simple catarrhal affections of the tonsil were under certain conditions (it might be atmospheric) infectious. He had also observed that tonsils already affected by catarrh were more liable to take on diphtheritic action when exposed to infection. This was especially the case in scarlatinal tonsillar catarrh. Gargles, he found, they could never get a child under ten to use, and he was not sure that they did much good. The best method of applying remedies was by painting or spraying, or by Kirby's glycochol lozenges, or by the insufflation of tannin or dried bicarbonate of soda.

2. TRAUMATIC STRICTURE: ITS TREATMENT BY SUPRA-PUBIC CYSTOTOMY.

By JOHN DUNCAN, M.A., M.D., F.R.C.S.E., Surgeon to the Royal Infirmary, Lecturer on Surgery, Edinburgh School of Medicine, etc.

It is well known that stricture of the urethra is most severe and dangerous when traumatic in origin. This arises from various causes. The wound which produces a stricture necessarily involves the mucous membrane of the urethra, and so substitutes for it cicatricial and contracting tissue, while a stricture from gonorrhœa may be strictly limited to the submucous tissue. In the former case healing takes place under direct irritation from the passage of urine, and consequently with a larger outpouring of inflammatory products.

Again, a much larger part of the circumference of the urethra is apt to be affected in traumatic stricture. This circumferential character of the injury is indeed of essential importance. If a longitudinal wound be made in any canal it is not prone to lead to diminution of its calibre. Perhaps the only exception to this lies in the air passages. After tracheotomy, and like operations, there is a tendency to lessening of the lumen, which may sometimes give rise to much trouble. This, however, comes from an exuberance of granulations—an over-growth, not a contraction of scar tissue. Last summer, in a girl of ten, who was sent to my ward by Dr M'Bride on account of extensive papillomata of the larynx, I opened the thyroid cartilage and cleared away the growths. A tube was kept in only for five days above the cricoid cartilage. Everything went admirably, and the patient was out of the Infirmary three weeks after the operation. In two months she came back with considerable and increasing dyspnœa, and on examination with the laryngoscope it was found that in the situation of the scar was a smooth, well-defined, sessile swelling, which greatly diminished the space for breathing, and which necessitated the performance of tracheotomy to keep the patient in safety. This curious tendency, then, of wounds in the windpipe does not invalidate the fact that longitudinal wounds do not lead to stricture in any of the passages in the body. It is an outgrowth, not a contraction. In the urethra, lithotomy, median or lateral, does not produce contraction, the surgeon opens the canal freely to remove foreign bodies, and even incises for the relief of a stricture which has, perhaps, been produced by a previous wound without the slightest fear of establishing thereby any or further impediment to the flow of urine. It is far otherwise with transverse wounds. Wherever they occur they tend to diminish the lumen. The tendency may be overcome in various ways, but it always exists. In the air passages it may be prevented by the rigidity of their walls. An interesting illustration

of the tendency and its prevention is to be found in preternatural anus. There the upper orifice is kept patent by the passage of the fæces, while the lower contracts, so that the entry even of a probe may be difficult. But the most striking example of the law is found in urethral surgery. The surgeon who amputates the penis must take special precautions against contraction of the orifice, and traumatic stricture is severe and dangerous precisely because it results from a transverse wound. The reason for this plainly lies in the fact that it is the new material which contracts. If the wound be longitudinal the resulting contraction is only from end to end, and the tube may be thereby so far shortened, but cannot be diminished in circumference. On the other hand, if it be transverse, contraction takes place in the length of the wound and so lessens the lumen of the passage.

But yet another cause is constantly in operation to render traumatic stricture a serious disease. It is usually produced by very violent contusion, and a considerable portion of the urethra and its surroundings is apt to be injured, contused, or irregularly torn. The formative material is therefore necessarily considerable, and the contraction commensurate with its quantity.

For these reasons, then, traumatic stricture is a serious condition, and its treatment at once important and difficult.

I wish to bring before the Society a short account of certain examples of this condition, which present some peculiarities, and one of which is necessarily very rare.

In the early stages of an injury of the urethra we have to fear present extravasation of urine as well as future stricture.

If a patient has received a severe injury in the perinæum, and has passed a quantity of blood but not yet made any attempt to urinate, care must be taken that no urine pass over the injured spot until the wound has so far healed that it will not sustain harm thereby. This is done by the regular passage of the catheter, with stringent antiseptic precautions. As a rule, this may be easily enough accomplished. Difficulties, however, arise from two causes.

1. The nature or form of the injury may present an insuperable obstacle. The soft textures may be so smashed that it is hopeless by any amount of groping in the dark to find the torn end of the canal, or the interposition of portions of mucous membrane or other tissue may oppose an entrance to the bladder. In such cases we have two alternatives—perinæal section and aspiration. I distinctly prefer the former. Its greater safety, in view of extravasation or abscess, and the probability that in any event we must ultimately resort to it, are powerful arguments. A patient still comes now and again to the Infirmary to have an instrument passed, who furnished a good illustration at once of this method of treatment and of the peculiarities met with in individual cases. Engaged as a carpenter in the construction of the Waverley Market Hall in this city, he fell from a height of nearly 20 feet

astride a beam. He was only brought to the Infirmary about six hours afterwards, on account of inability to pass water. I found his perinæum enormously distended with extravasated blood, and, failing to pass a catheter, with the assistance of my colleague Dr Maclaren I made an incision in the middle line. The posterior end of the torn urethra was with difficulty found after careful search. I was still unable, however, to reach the bladder, and on introducing my finger into the rectum, found that the obstacle lay in a greatly enlarged prostate. Various forms of catheter were tried without success. It was imperative to relieve the bladder immediately; and so, for the only time in my professional experience, I forced the prostate and tied in a catheter. Even in this case, then, I did not aspirate, being influenced by the consideration that in perforating the prostate I did not add greatly to the severity of the wound, and that I established a route to the bladder which might be useful in the future.

2. It may become impossible to pass a catheter on account of the inflammatory swelling which invariably comes on soon after the injury. In such cases I have on several occasions aspirated with complete success. If the catheter have been successfully passed at first, the chances are that you will again succeed when the inflammation subsides. Sometimes these cases do well without aspiration. I was called two years ago to see a patient in Fountainbridge who had ruptured his urethra by a fall. For three days I easily and regularly drew off the urine. On the fourth day I failed with the same catheter, which had hitherto passed with ease. I went home to procure other instruments, among them the aspirator; but on my return found that he had been unable longer to restrain himself. Urine had flowed freely, but with much pain. No evil whatever followed; and but for a stricture which requires occasional attention, the patient is perfectly well. Such exceptional good fortune is not, however, to be relied on. If the catheter be left off even after a week or ten days, it is apt to be followed by perinæal abscess. I have not after a week met with direct and immediate extravasation; but abscess is in my experience common, and if not incised early may, of course, become diffuse.

These early difficulties overcome, the surgeon is brought face to face with a tendency to contraction so strong that it requires the utmost skill and patience to overcome it. But with patience stricture may be prevented by the regular passage of bougies every few days. Very frequent, even daily, use is sometimes necessary just at the period of fibrination, and with most the occasional passage of an instrument is required for the rest of life.

It frequently happens, however, most often from negligence on the part of the patient, that the stricture is fully formed ere the surgeon has to deal with it. In principle the treatment then differs in no respect from that of the other variety of stricture. In practice the more severe forms of operation are more often

required. Success with ordinary dilatation is not so certain as in gonorrhœal stricture. When Holt's instrument can be passed, it will generally serve our purpose. But in one case I found it impossible to tear the stricture with the utmost force, and was obliged to resort to internal urethrotomy, while in others the urethra is so completely destroyed that perineal section in one form or other is imperative. The following case may serve to illustrate the difficulties which sometimes are met with. The operation devised is one which, although adapted to but a small number of cases, in them would seem likely to be useful and satisfactory.

John Dunbar, æt. 28, a crofter from Caithness, was admitted to the Royal Infirmary on 21st January 1883. His medical history, family and personal, was good, and although somewhat thin and pale, he was generally in sound health.

On the 2nd of May 1882 Dunbar, with five or six others, was engaged in launching one of the large and heavy boats in which the herring fishery is usually pursued. The attitude in which the traction is made is with the back towards the boat. By some want of simultaneous action the boat was allowed to heel over, and Dunbar was crushed to the earth, the swell of the boat's side falling on his buttock.

It is difficult to make out the precise sequence of events after this. It would appear, however, that he had first retention of urine, with escape of blood from the penis. The catheter was successfully used for a few days, but being then discontinued because he could pass his water, perineal abscess followed. After that time he passed all his urine through the perinæum.

On admission there was found to be a large orifice anterior to the scrotum, from which a passage led into the urethra somewhat farther back. The perinæum was a mass of densely-indurated texture, with at least half-a-dozen fistulæ, from which the urine poured as from the rose of a watering pan when he made water. It is worthy of remark that there was practically no cystitis. The patient was able to retain his urine for two or three hours during the day, and was little disturbed at night.

Repeated and patient attempts were made to pass instruments into the bladder, but without success. Occasionally, both by the urethra and through the fistulæ, flexible bougies of small size could be made to enter to the full extent, but with the finger in the rectum it could be ascertained that the course they took was to the right of the mesial plane, and farther distant from the bowel than the normal urethra.

On the 13th of March, having come to the conclusion that the stricture was truly impermeable, I passed a full-sized bougie as far as it would go—that is, a short distance behind the scrotum—and by an incision in the middle line of the perinæum reached its

point. I then passed a silk stitch through the divided mucous membrane of the urethra on each side, and proceeded to dissect carefully backwards. Soon the mucous membrane entirely disappeared, and only the tracks of the fistulæ could be seen. By passing probes it was ascertained that several of these communicated with each other. Following this main route, we were ultimately led to a cavity of considerable size, containing a little purulent urine and a piece of necrosed bone about the size of a pea. This cavity lay immediately behind the symphysis pubis, but perhaps a very little to the right. I now made a most careful search for the orifice by which the bladder communicated with the wound, systematically pushing the probe against every portion of the posterior and lateral surface of the incision. All the fistulæ communicated with the wound, but I was unable to detect the route thence to the bladder. I therefore passed a drainage tube into the abscess cavity, and sent the patient to bed.

On two subsequent occasions, when granulations covered the wound, I made farther nugatory attempts to reach the bladder. On the failure of these I contented myself with the regular passage of a full-sized bougie to the cavity behind the pubes, and ultimately the wound in the perinæum entirely healed. The patient was now so free from inconvenience, passing water partly per urethram, partly by the orifice at the root of the penis, that he left without permission on the 14th of May to escape the regular passage of instruments on which I insisted, and the closure of the sole remaining anterior fistula, which I purposed doing by a plastic operation.

Dunbar came back to us on 4th November 1883, or six months after going home. He had remained perfectly well till ten days before, *i.e.*, 25th October 1883. Pain was felt on that day, he thinks in consequence of a strain. He was unable to pass water for sixteen hours, till on the morning of the 26th it "burst," to use his own expression, through the perinæum. On admission a fistula was found at the posterior part of the perinæal cicatrix. He passed urine partly through it, partly by the anterior opening. He had suffered so severely in this attack that he was willing to submit to any means of cure.

After another failure to reach the bladder with the catheter, and in view of the result of the former operation, I determined to proceed differently. The patient was directed to retain his urine for two hours before the time of operation on the 14th November. By an incision in the middle line I easily reached and opened the bladder above the pubes. I found that a bougie passed into the urethra from within traversed the prostatic portion of the urethra and was then stopped. I directed it to be held in that position. Another bougie was then passed by the penis as far as it would go. It only remained to bring the two into contact, which I quickly did by cutting down upon the latter, and then, guided by a finger

in the rectum, dissecting backwards till I came to the point of the former. A full-sized catheter was then passed into the bladder and tied in position. An indiarubber drainage tube was placed in the supra-pubic wound without entering the bladder, and two or three stitches brought the rest of the abdominal wound together. The wound in the bladder had been made only large enough to admit the finger, and came absolutely together without stitches when the bladder contracted, so that I thought sutures in it would be merely a source of irritation.

The catheter was kept in the bladder for a week, and daily washings with a one per cent. solution of carbolic acid were made through it. After its withdrawal a No. 12 bougie was passed every third day. The supra-pubic wound was healed on 12th December, the perineal on 17th December; and the patient, being absolutely well in every respect, again went away without permission on 24th December, probably with a view to Christmas festivities at home.

Prof. Chiene rose to say how interested he had been in hearing a full account of this case, part of which Mr Duncan had related to him already. He thought the practice was one which might be adopted in similar cases. He thought that when one failed to pass the catheter, after having cut down on the stricture, he might be guided to the spot where the urethra was by causing the patient to micturate. In regard to the treatment of extravasation, he should like to know if Mr Duncan was still of opinion that it was best to incise the perineum. For his own part he preferred aspiration, because he did not think that extravasation should naturally lead to sloughing. In one instance where he failed to pass the catheter, extravasation having occurred, he aspirated the perineum three or four times, and was ultimately able to pass a catheter. It was putrid urine that caused sloughing, the urine becoming putrid from dirty instruments being passed. In two cases he had used aspiration with success. He had been taught that incisions were necessary, and had not dared to try aspiration before experimenting, by injecting urine into the cellular tissue of the rabbit, a tissue that was very prone to slough.

Mr Joseph Bell thought there could be no two opinions in the Society as to the excellence of Mr Duncan's paper. With regard to the question of the first treatment of a case where there was reason to believe traumatic rupture of the urethra had taken place, he still held to the opinions he had expressed in a paper read some little time ago to the Society. If the case was got in time before extravasation occurred, he believed that successive aspiration would cure the case. This was more particularly important in cases of fracture of the pelvis, because when an incision was made, the fracture became compound. As to the effect of urine on the cellular tissue, he had come to the same conclusion as Mr Chiene a great many years ago, but without experiment. He had

operated on a child for a large stone, and had thoroughly lacerated the side of the prostate gland, and there was no doubt urine was effused over the tissues, and yet there was no trouble. While agreeing theoretically with Mr Chiene, it had not been his lot to see cases of extravasation go so kindly, probably from the catheters not being thoroughly antiseptic. In a case seen first by his house-surgeon, the catheter was passed with most elaborate antiseptic precautions, and though there was undoubted evidence that the urethra was ruptured, it reached the bladder at once. A tube running into a vessel half full of a carbolic solution was fixed to the catheter, but one day a clot having formed in the passage, the patient strained himself so hard as to drive out the catheter, and make water naturally. A perineal abscess followed, but this disappeared after incision, and he trusted that stricture would not result, bougies being passed very readily, and with great ease. With reference to Mr Duncan's ingenious method of treatment in the case he detailed, Sedillot of Strasburg, he thought, had proposed a similar mode of treatment for such cases, but he was not aware that he had ever performed it.

Dr MacGillivray said he had seen the very operation described by Mr Duncan performed by M. Pean in St Louis Hospital in 1874. Perineal section was first performed, but the upper end of the urethra could not be made out. The bladder was then opened above the pubis, and an instrument passed from within outwards. He could not say what the result was.

Mr A. G. Miller wished to ask Mr Duncan if he considered that traumatic rupture of the urethra was always transverse, and if so, why? He did not think that in every instance rupture of the urethra caused by fall with the legs astride of anything was always transverse. Mr Chiene seemed to be of the opinion that sloughing after extravasation was due to only one cause, and that was sepsis. He thought it would be safer to say that the sloughing was due to inflammation, and the inflammation to the sepsis.

Mr John Duncan, in reply, said that there were various important points in connexion with traumatic stricture that were still undecided. One of these was the question of sloughing after extravasation of urine. He thought Mr Miller had put the case very well, when he said that it was due to inflammation, and the inflammation to sepsis. Sloughing did not always occur after extravasation, but it was never desirable that a man should pass water into his cellular tissue. Urine was very variable in its irritating qualities. It might lead to nothing or only slight inflammation, but it might also cause inflammation to such an extent as to be followed by abscess and sloughing. The question was, how were they to prevent a man passing water into his perineum? That was an important point. He agreed with most of the speakers that in a certain number of cases aspiration was sufficient. In other cases a free incision was, he thought, the

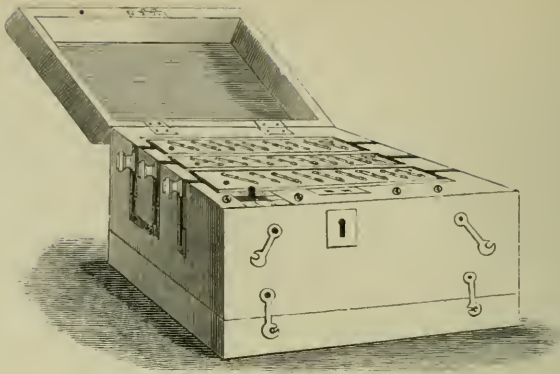


FIG. 1.

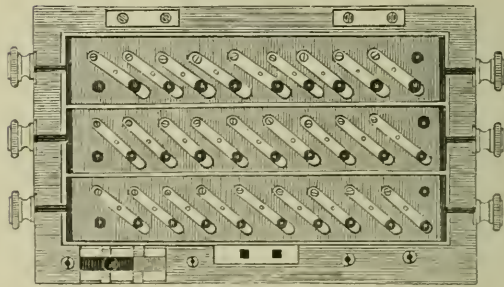


FIG. 2.

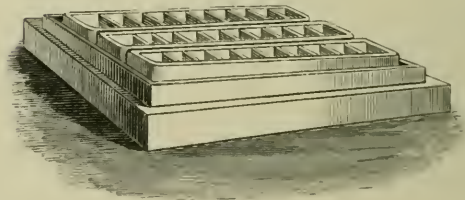


FIG. 3.

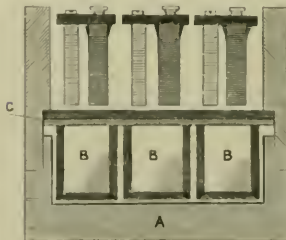


FIG. 4.

DR. A. BRUCE'S NEW FORM OF PORTABLE GALVANIC BATTERY.

best mode of treatment, more particularly as it was not improbable that attempts to pass the catheter would fail without assistance from a perineal action. He had no doubt that rupture of the urethra in the usual situation was always transverse. It was the jam between the outside force and the bone that caused the nip, and the bone lying transversely across the urethra must cut it transversely also. A sharp instrument cutting from the outside might make a longitudinal incision. He thought the supra-pubic operation on the bladder might be extended to many other conditions, as lithotomy. He had done it lately in preference to lateral lithotomy. It avoided the risks of hæmorrhage, and of injury to the prostate, and there was less depth of tissue to cut through. There was little fear of opening the peritoneum so long as the operator kept well behind the pubis.

Meeting IX.—July 2, 1884.

Dr H. D. LITTLEJOHN, *President, in the Chair.*

I. EXHIBITION OF INSTRUMENTS.

Dr A. Bruce showed a new form of portable GALVANIC BATTERY. The box (Fig. 1) measures $7\frac{3}{4}$ inches long, 5 inches broad, by $4\frac{3}{4}$ inches deep, and with its contents weighs only $4\frac{1}{2}$ pounds. The box consists of two parts, an upper and a lower (Figs. 1 and 4), of which the lower contains the cells (Figs. 3 and 4), and the upper (Figs. 1, 2, and 4) the elements. The cells (Fig. 3, and seen in antero-posterior section in A, Fig. 4) consist of three vulcanite trays, each containing ten compartments. The depth of each compartment is $1\frac{1}{4}$ inches, its width $\frac{1}{2}$ inch, and its breadth 1 inch; the thickness of its wall $\frac{1}{8}$ inch. These three trays are placed side by side, as in Fig. 3, in the lower segment of the box, the whole interior of which is coated with a waterproof cement to prevent injury to the wood from any accidental spilling of the liquid. When the battery is not in use the cells are completely closed by a plate of vulcanite, to the under surface of which is fixed a sheet of vulcanized rubber, sufficiently soft to be pressed into the cells when the box is shut. It is kept in position by the pressure of the projecting part of the wall of the upper segment of the box (see Fig. 4), and so close is its apposition to the cells that the box may be thrown about in any way without the escape of a drop of fluid from the cells. The elements, zinc and carbon (Fig. 4), measure $1\frac{1}{2}$ inches long by $\frac{1}{4}$ inch square. They are attached to three vulcanite bars (Fig. 2, where they are seen from above) by means of screws. When broken or worn they can be easily replaced

by others. The zinc of one cell is connected to the carbon of its neighbour by means of a strip of brass (seen unshaded in Fig. 2), in the centre of which is a round aperture into which the brass plug of the "collector" fits. From each extremity of each vulcanite bar a brass limb passes through a slit in the end of the box (Fig. 1). By means of screws on these brass limbs the vulcanite bars can be raised, lowered, and fixed in any position. The sides of the slits at the end of the box are guarded by brass plates, which serve the double purpose of strengthening the woodwork of the box and of making connexion between the elements of adjacent rows of cells. The accessories, wires, electrodes, handles, etc., are carried in the lid (not shown in Fig. 1). A commutator is placed in the front wall of the box (Fig. 1, to left of lock). The fluid used is—

| | |
|------------------------|------------|
| Water, | 150 parts. |
| Bichromate of potash, | 10 " |
| Bisulphate of mercury, | 10 " |
| Sulphuric acid, | 5-15 " |

From one to two drachms in each cell will be found sufficient for use, and will last for several applications of the battery. When it is used up it must be thrown away and replaced by fresh fluid. If the battery is not in constant use it is advisable to throw out the fluid and let the cells stand empty. It will be found that for ordinary use from seven to ten cells are sufficient. To sum up, the battery is very portable; there is no danger of fluid being spilt during transport; it is very simple. Every part can be readily inspected, any flaw detected and repaired by the owner of the battery with the greatest ease.

II. EXHIBITION OF PATHOLOGICAL SPECIMENS.

1. *Dr Claud Muirhead* showed (a) the BRAIN OF A PATIENT WHO SUFFERED FROM APHASIA AND LEFT-SIDED HEMIPLEGIA WITH LESION ON THE RIGHT SIDE; (b) the HEART of the same patient. In the heart the mitral orifice was greatly contracted. There was in addition a thrombus in the left auricle as large as a pigeon's egg, and laminated, attached by a pedicle, so that it hung tolerably free in the auricle. The right heart was greatly hypertrophied. The pulmonary artery was atheromatous throughout to its minutest branches. During the course of the illness there developed a curious murmur, peculiarly harsh, rough, and grating, and at times musical. This was evidently due to a rupture of a segment of the valve, or of one of the chordæ tendinæ, or to some fibrinous deposit on the valve. It turned out that the whole orifice was over-laid by three tongues of lymph on the auricular surface. The murmur was thus produced by the regurgitating stream throwing these tongues into vibration. When getting out of bed one day the patient suddenly fell down unconscious. He

became aphasic and hemiplegic. An embolism had been carried to the brain. The peculiarity was that the hemiplegia was on the LEFT side of the body. This was all the more unusual, as the man had always been right-handed. He practically recovered his speech, the hemiplegia disappeared in great part; but he developed a weakening of intellect, and ultimately died. On examination post-mortem the RIGHT side of the brain was found to be much softened. The embolism had been carried into a branch of the right middle cerebral artery. The softening that occurred was well shown in the sections made.

2. *Mr A. G. Miller* showed PORTIONS OF THREE RIBS removed to favour contraction after empyema. Contraction had stopped after going on to a certain extent. The ribs were found to be closely in contact with each other. It was therefore thought advisable to resect portions to admit of further contraction. The parts removed were greatly thickened on their inner surface. The empyema had caused a sufficient amount of periosteal irritation to lead to the development of new bone. He would like to ask if any example of this had been met with in cases of long standing empyema. After the resection contraction went on rapidly, the cavity being reduced to one fourth or fifth of what it had been previously.

3. *The President* showed (*a*) a RUPTURE OF THE DIAPHRAGM and rupture of the heart from the body of a woman *æt.* 70, who had fallen a height of from 50 to 60 feet. The cardiac end of the stomach was found in the thoracic cavity, having formed a hernia through the rupture in the diaphragm. The heart was ruptured at the base, the usual spot where this injury occurred in falls from a height. It was curious to have this case following so closely on a similar one he had shown at the previous meeting. How the old woman had chanced to fall was not known. She was said to have been given to somnambulism, but of that they did not have distinct proof. She had, however, been greatly struck by the suicide of the previous month. (*b*), The parts from a case of suicidal CUT-THROAT, with the instrument by means of which the injury was inflicted. The case occurred in the person of a gentleman who had been affected by insanity and showed suicidal tendencies. He appeared to get better. One day he left the room during dinner. He was followed, but before he could be intercepted, had made his way to a part of the house where some work was going on, and cut his throat with an ordinary joiner's saw which had been left with some other tools by the workmen. Tracheotomy was performed. He lived two days. The case was interesting from the unusual nature of the instrument.

III. ORIGINAL COMMUNICATIONS.

1. THE EARLY STAGES OF TUBERCULOSIS.

By G. A. GIBSON, M.D., D.Sc., F.R.S.E., Lecturer on Materia Medica and Therapeutics, and Tutor in Clinical Medicine in the Edinburgh Medical School.

As the aim of this paper is limited to the study of some of the clinical aspects of commencing consumption, it does not of necessity concern itself with the consideration of pathological doctrines. It would nevertheless be at once matter of interest and profit to cast a brief glance, did time permit, upon some of the various phases of opinion held by those who have gone before us. The definition of tubercle by Morton, which formed the dawn of a new era; the careful researches of Baillie, leading him to the belief that there were two forms of phthisis, tubercular and non-tubercular; the wide generalization of a tubercular diathesis by Bayle, who, however, went astray in one direction, and paved the way for the opinion that there could be but one form of phthisis,—a doctrine supported by the overwhelming authority of Lænnec, and accepted throughout Europe by almost every prominent writer, save Autenrieth, Schönlein, and Addison; the return to the dualistic views of Baillie and Vetter, effected mainly by the master mind of Graves, under whose guidance the unitarian dogma tottered to its fall; the more recent developments, foreshadowed by the brilliant induction by Villemin of the communicability of tuberculosis, and culminating in the achievements of Koch,—these and many other modes of thought might usefully engage our attention were they within the sphere of this communication.

For us, as practical physicians, however, it is impossible to pass over the modern change of opinion as to the prognosis in consumption—the phase of hopefulness, if such a term may be given to it. This paper does not touch upon pulmonary phthisis, commonly so called, and nothing can be said about the useful work of Rogée, and many others who led the way to the now universal belief in the frequent cure of wasting lung disease. But with regard to tuberculosis, with which we are now concerned, it must be remembered that Lebert has been instrumental in modifying the hopeless outlook which was, until twenty years ago, the only prognosis given in such cases. He narrated the appearances found in four autopsies of persons who had died of other diseases, and who presented absolute evidence of cured disseminated miliary tuberculosis, and described two other cases, whose symptoms were undoubtedly tubercular, in which the disease was cured in his own hands. Since the date of his publication, well-nigh twenty years ago, many others have corroborated his statements in this country, especially M'Call Anderson, so that we are justified in giving a less gloomy, if still grave, prognosis in cases of tuberculosis.

This paper is prompted by a case which has recently been under my observation, and which seems to support the more hopeful views of our own times.

On the 24th February of the present year a gentleman, belonging to a seaport town in the north of England, came to consult me, complaining of a feeling of lassitude and feverishness, with copious perspirations every night.

History of Present Attack.—The first symptom was noticed early in January, and this was nightly perspiration. In the beginning of February a feeling of fatigue and lassitude began, which culminated about the middle of the month in extreme depression. At the same time he observed a loss of flesh. He consulted his medical attendant, who found the temperature 104° Fahr., and gave him medicine, which produced frightful purgation—I use the patient's own words—but lowered the temperature, and checked the perspiration, at the same time causing anorexia. When it was done the appetite returned, but the perspiration also recurred with the high temperature. He struggled on for a week or two, and then came to Edinburgh.

The previous history is on the whole satisfactory, but there is one fact which may have some bearing on the subject of this communication,—this being that for a good many years he has suffered every June and July from hay-asthma. With this exception he has had no illness, save the usual children's ailments.

The social history is in every way good, unless the life of a practical chemist is to be regarded as unhealthy. My patient has prosecuted some inquiries in this direction without finding any fact pointing to such a conclusion. For eleven years he has been in various laboratories connected with manufacturing chemists, chiefly makers of artificial manures. Perhaps our President will kindly inform us if he has in the course of his experience met with any instances in which such an employment has been prejudicial to health.

Family History.—The patient's father is a strong man of middle size, aged 64, who has enjoyed very excellent health until the last two or three years, during which he has suffered from bronchitis in winter. His mother, aged 64, has been a very active woman, but is now subject to some affection, said to be connected with the heart. There are two brothers, aged 34 and 27, the elder of whom is troubled with sore throat, and the younger has not been very strong since an attack of diphtheria with a severe relapse, and subsequent paralysis. The patient has three sisters, aged 36, 31, and 23. The eldest has never been troubled by any ailment, and the youngest has also been at all times in good health, but the second had a fall in childhood over the bannisters, and is in somewhat delicate health, with a tendency to convulsive seizures. There have been no deaths in the family, which, although not robust, has certainly no apparent hereditary taint.

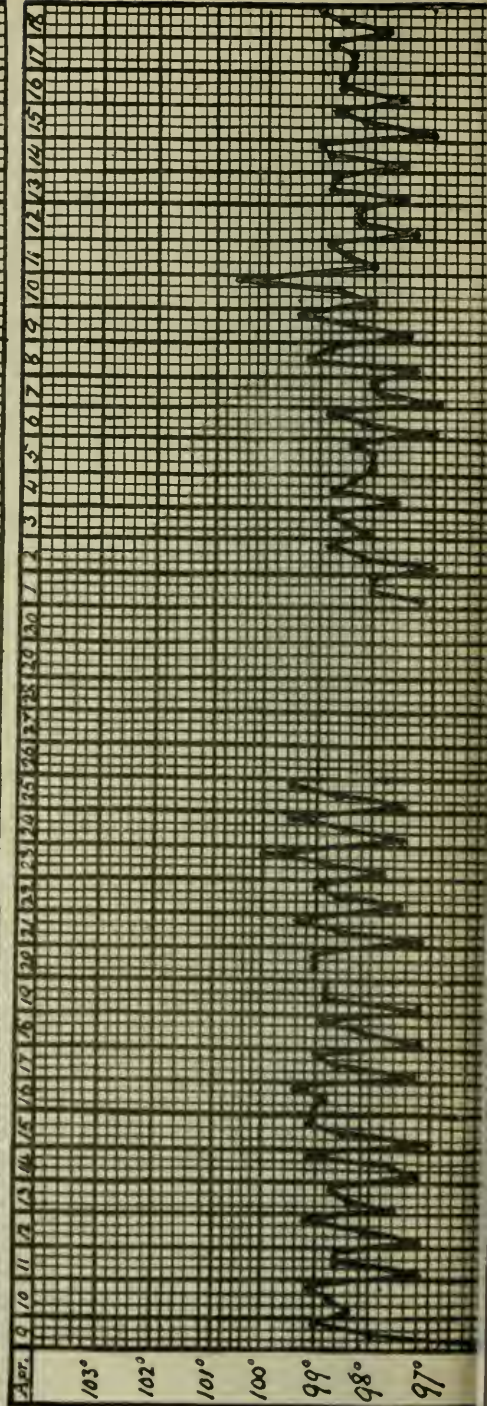
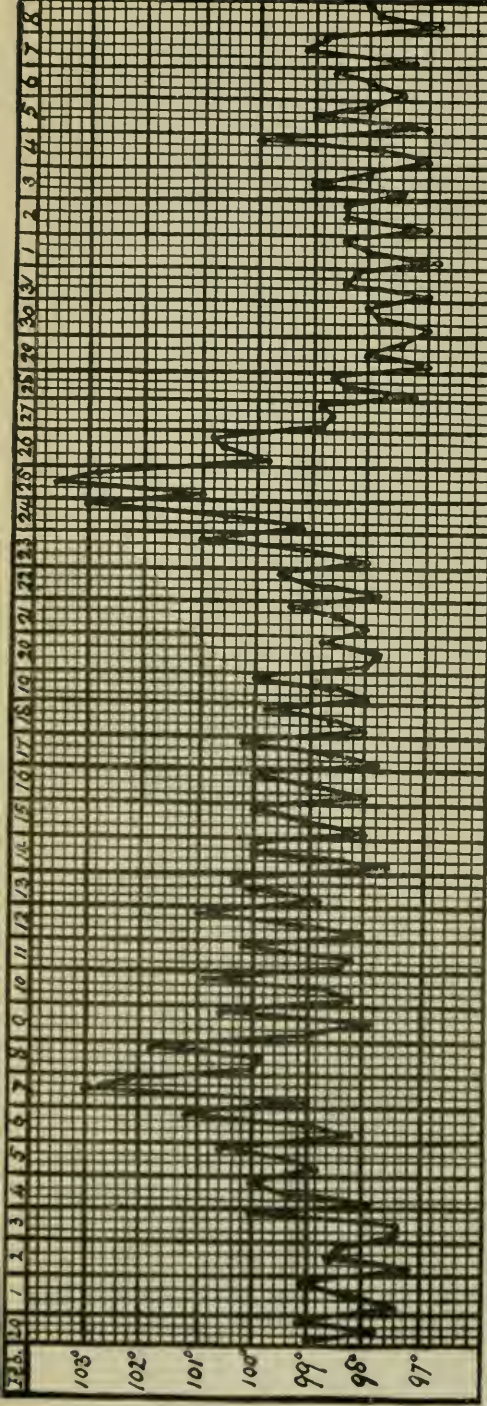
Condition when first seen.—Age 29; height 5 feet 7½ inches; weight about 9 stone—with fair complexion, gray eyes, and light brown hair. The face was rather thin, the bony prominences being unduly distinct, and the cheeks pale, with a bright spot on each side. The lips, gums, and palpebral conjunctivæ slightly blanched, and the sclerotic somewhat more than usually transparent. The tongue was flabby, and indented by the teeth, with a moist creamy fur behind. The skin was damp, and the muscles soft. The temperature was 102° Fahr. in the axilla. Of the pulse there is no note. On inspecting the thorax its form was seen to be normal, and its movements free. The blue tint of the veins could be seen through the skin more clearly than usual. There was no increase of the vocal fremitus to be found on palpation of either side. On percussion, however, there was a slight change in the note in the infra-clavicular area of the right side. There was no definite change in the pitch, but it was muffled as compared with the left side. On auscultation the breath sounds over the upper part of the right lung were found to be harsher than over the corresponding part of the left, and in the infra-clavicular and supra-spinous areas there was faint fine crepitation. There was not the least increase of the vocal resonance. The cardiac area was confined to its usual limits, and the sounds were healthy although the action was frequent. The position and extent of the abdominal organs were normal. The urine was measured a few days later, and found to amount to 20 fluid ounces per day. It was reddish yellow in colour, and highly acid, with a specific gravity of 1026. It contained no abnormal constituents, and gave, on quantitative analysis, 15·84 grains of urea per ounce, or 316·8 grains in twenty-four hours.

After thorough consideration and careful exclusion of every other possible disease the conclusion was reached, which seems unavoidable, that the patient was in an early stage of tuberculosis. The following mixture was prescribed:—

| | | | | |
|---|--------------------------|---|---|----------------|
| ℞ | Liquoris atropiæ, | . | . | min. xij. |
| | Quiniæ sulphatis, | . | . | gr. lx. |
| | Acidi sulphurici diluti, | . | . | fl. dr. iij. |
| | Aquam, ad, | . | . | fl. oz. vj. M. |

Sig. A tablespoonful in water three times a day before food, and the food was ordered to be at once simple and generous.

Nothing would persuade him to remain in Edinburgh beyond the next day, but he promised to return in three days, and to stay for a time under observation; he was therefore reluctantly allowed to go home to attend to some necessary business, although with some misgivings as to the prudence of permitting a man to go about with a temperature of 102°. He returned on the 28th, when arrangements were made for the registration of the temperature every two hours, and for the measurement of the urine, a specimen of which was daily tested for urea. His stay in Edinburgh was



continued until the 11th March, beyond which date he found it impossible to prolong it. He continued to take the medicine which has been spoken of, and the same description of food, and if the weather was good, and his temperature not above 100° , he was allowed out a little every day.

It were tiresome to follow all the details of his progress, and only a rapid sketch of it can be given here, which is filled in by facts noted on the temperature chart. For the first three days after his return the temperature was tolerably low, and the pulse did not exceed 80, but he perspired copiously every night. For some days afterwards, however, the temperature was high, and the pulse varied between 90 and 100, so it was deemed advisable to order him the following:—

| | | |
|---------------------------------|---------------------------|----------|
| ℞ | Quiniæ sulphatis, . . . | gr. xij. |
| | Pulveris opii, . . . | gr. vj. |
| | Pulveris digitalis, . . . | gr. iij. |
| Misce et divide in pilulas xij. | | |

Sig. One to be taken three times a day after food.

As already said, he returned to his home in the north of England on the 11th March, before which date the crepitation had disappeared and the harshness of the breath sounds became much less noticeable. Before permitting him to go home a promise was exacted that he would on no account attend to business more than two hours on any one day, and that he would, when in the chemical works, use a respirator with eucalyptus oil, in case any irritation of the pulmonary mucous membrane should be caused by the fumes given off during the chemical processes.

If we turn to the temperature chart, which begins with the 29th February, and on which the morning, midday, and evening temperatures are recorded, we see that there is a general tendency shown to a rise at night. On the 1st, 2nd, 3rd, and 4th March the morning temperatures were below normal, and until the 3rd the evening rise was not much above normal, but from that date it rose considerably, culminating in a temperature of 103° at midday of the 7th. It came down after that date, but the evening record was always 100° or more until the 18th, when it was lower; next night, however, it was again 100° , but on the three succeeding evenings it was $98^{\circ}\cdot 8$, $99^{\circ}\cdot 4$, and $99^{\circ}\cdot 6$. The following morning, that of the 23rd March, he somewhat incautiously took a hot bath, which he blamed for a rise of temperature which began on that day, and reached its climax in the middle of the 25th, when there is a record of $103^{\circ}\cdot 6$. On the evenings of the 26th and 27th the temperature was $100^{\circ}\cdot 8$ and $98^{\circ}\cdot 8$. The low morning temperature readings again began on the morning of the 28th, and with the exception of the 10th and 16th April continued until the 25th, when unfortunately the thermometer was broken and could not be replaced for some days. Sometimes the morning temperature was

extremely low, as for instance on the 9th, when it only reached 96°·2.

Since the beginning of April the evening temperature has only three times reached 100°, on the 4th and 23rd April when it was 100°, and the 10th May when it rose to 100°·6.

During the first half of May there was a gradual return to a steadier temperature, the morning records, however, being as a rule too low. From the 1st to the 18th the temperature only five times reached or exceeded 99°, and since the 18th it has never reached 99° except once, on the 22nd of that month. In like manner the pulse gradually became less frequent, diminishing from the extremes of 80 and 100 to the more moderate limits of from 65 to 75.

Until the 16th March the patient perspired profusely every night, but on the 16th and 17th he was free from night-sweats, which returned the two following nights. On the 20th, 21st, 22nd, and 23rd he was again free of them, but on the 24th, when there was a rise in temperature after an imprudent bath, there was a return of the perspiration, which recurred every night until the 30th, being, however, considerably diminished on the 28th, 29th, and 30th. Since that date there has not been a night-sweat.

The daily quantity of the urine and of the urea can be seen at a glance from the following table:—

| | Sp. gr. | Urine in oz. | Urea in gr. | | Sp. gr. | Urine in oz. | Urea in gr. |
|---------|---------|-----------------|----------------|----------|---------|-----------------|----------------|
| Feb. 29 | 1026 | 20 | 316·8 | March 14 | 1014 | 67 | 412·72 |
| March 1 | 1020 | 35 | 400·4 | " 15 | 1017 | 54 | 356·40 |
| " 2 | 1022 | 49 | 517·44 | " 16 | 1016 | 77 | 406·56 |
| " 3 | 1026 | 50 | 682 | " 17 | 1015 | 72 | 443·52 |
| " 4 | 1021 | 48 | 443·52 | " 18 | 1016 | 79 | 421·40 |
| " 5 | 1019 | 62 | 463·76 | " 19 | 1016 | 68 | 463·76 |
| " 6 | 1020 | 57 | 476·52 | " 20 | 1015 | 78 | 411·74 |
| " 7 | 1020 | 49 | 431·20 | " 21 | 1016 | 76 | 401·28 |
| " 8 | 1021 | 46 | 465·52 | " 22 | 1015 | 78 | 377·52 |
| " 9 | 1017 | 52 | 457·60 | " 23 | 1017 | 78 | 480·48 |
| " 10 | 1021 | 41 | 396·88 | " 24 | 1017 | 71 | 468·60 |
| " 11 | 1021 | not measured. | | " 25 | 1016 | 64 | 450·56 |
| " 12 | 1022 | 46 | 485·76 | " 26 | 1017 | 62 | 672·88 |
| " 13 | 1015 | 63 | 415·80 | " 27 | 1020 | 56 | 492·80 |

From this date the analysis of the urine was discontinued. Along with the improvement in his general condition the patient gained weight steadily. On 13th March, two days after his return to England, his weight was 9 stone 1 lb. A week later it was exactly the same, but the following week it began to increase. The weight is given below with the dates on which it was taken.

| | | | |
|----------|-------------|----------|---------------|
| March 13 | 9 st. 1 lb. | April 24 | 9 st. 12½ lb. |
| " 19 | 9 " 1 " | May 2 | 10 " — " |
| " 27 | 9 " 2 " | " 8 | 10 " 1½ " |
| April 3 | 9 " 9 " | " 15 | 10 " — " |
| " 10 | 9 " 9 " | " 22 | 10 " 1½ " |
| " 17 | 9 " 10½ " | " 30 | 10 " 2½ " |

During the preceding month of June the weight has kept steadily at the last figure or slightly above it. Save for a slight return of hay-asthma, his condition has been very satisfactory lately; he eats and sleeps as well as formerly, performs his duties fully, plays tennis a great deal, and takes much out-door exercise of all sorts, especially on horseback—in fact, he writes that he is as well as he ever was in his life.

At present he takes small doses of quinine, and makes use of cold sponging and subsequent friction. There seemed to be no reason for alarm more especially on account of his lungs than for any other organ, but as there is undoubtedly such a condition as chronic pulmonary consumption of tubercular origin, dumb-bell exercise has been recommended, for if the current theory be correct, that the elective seat of disease is due to want of expansion of the upper portions of the lungs, it follows that shoulder exercise must be one of the best preventives of the morbid change.

The aim of this paper is to make use of the case narrated in order to advance the propositions that there is in disseminated tuberculosis a distinctive period which precedes the outbreak of the graver symptoms, that this early phase of tuberculosis ought to be better recognised than it is, and that it is curable.

The President said that, as far as he knew, they did not find any proclivity to phthisis in those who worked in chemical manures. It might be that the fumes of sulphurous acid, which was so much used, had a protective effect. As to the curability of tubercular phthisis, he could only say that the bodies he opened in the public mortuary which did not present some evidence of a previous phthisical attack were few indeed.

Dr Muirhead thought the whole question in the case presented by Dr Gibson was one of diagnosis. Whether it was truly a case of tuberculosis or not was one for consideration. The point, however, which Dr Gibson wished to dwell on was the curability of acute tuberculosis at an early stage. He himself had always had a belief that cure was possible even at a further advanced stage of tuberculosis; but as his views differed from those generally held, he had hesitated to push them. He thought the cure was to be obtained by the free use of iodide of potassium, which drug he considered had been of distinct benefit in some cases under his care.

Dr Ireland said that as men grew older their views perhaps became more sombre; but he and his fellows under Hughes Bennett certainly began their careers with much brighter hopes than those of their young friend Dr Gibson. He thought that consumption and tuberculosis were curable at the outset and in the early stages. Twelve years ago he paid great attention to chest diseases, and went to Madeira for some time, and saw many cases of consumption there. He could recall four cases at least

where the disease must have been more advanced than in the case described by Gibson. They were cases in the second stage, in which some destruction of lung had occurred, but the disease had been completely arrested. Their young friend had taken too desponding a view to commence with, but he hoped to see him have brighter hopes as he grew older.

Dr Woodhead mentioned that, along with *Dr Gibson*, he had endeavoured to collect the breath in this case to examine it for bacilli. They had not adopted the ordinary method of making the patient use a respirator of gun-cotton, but asked him to breathe through a tube into two sterilized flasks, one containing serum, the other gelatine. The results were negative, but the method was not good.

Dr Byrom Bramwell would like to express the pleasure with which he had listened to this ably recorded case. *Dr Gibson* had advanced the proposition that tubercular phthisis was curable, a point on which *Bennett* had insisted long ago. But the real point of importance was, when they had a patient in the early stages of tuberculosis, could they say that they could cure him. He thought they could do so yet. Post-mortem examinations showed that tubercular phthisis was a curable disease; but he would be a bold man who would say, after having made a diagnosis of early phthisis, that the patient would get better. Some patients died in spite of the best possible form of treatment. With regard to the point mooted by *Dr Muirhead*, the curability of general tuberculosis, there were certainly some cases of this affection that were curable. He had himself seen recovery in several instances from tubercular meningitis. *Dr McCall Anderson* had also recorded several illustrations of cure of tubercular peritonitis.

Dr Carmichael would like to inform *Dr Gibson* that he had seen a good many times a very similar clinical history in the child. The same question occurred to him whether they were cases of incipient tuberculosis cured. The difficulty was to prove whether they were dealing with tuberculosis or not. He had seen and had records of cases very similar in which the child was cured. One case occurred last winter. The temperature was very high. There were no physical signs whatever, but there was the peculiar form of pyrexia met with in tuberculosis. So far as he knew, the child was still alive. The question was a difficult one to consider, but he hoped it would be solved by further investigation.

Dr Gibson returned thanks to the President and other members who had made remarks on his paper. He agreed with *Dr Muirhead* as to the difficulties met with in diagnosing early stages of tuberculosis, and was happy to know that *Dr Muirhead* held such hopeful views as to the curability of the disease. He feared that their *old* friend *Dr Ireland* was but imperfectly acquainted with recent progress in regard to phthisis and tuberculosis, to the latter of which alone the paper referred. He wished especially to thank *Dr Woodhead* for his assistance and for his remarks.

2. GERMS AND THE SPRAY.

By JOHN CHIENE, F.R.C.S. Ed., F.R.S.E., Professor of Surgery in the University of Edinburgh.

THIS experimental investigation has been undertaken in order to fulfil a promise which I made to this Society when Mr Duncan read his paper entitled "Germs and the Spray" on the 7th of February 1883. My reason for making that promise was that his experiments seemed to me to require repetition, because the results were different from the results of a series of experiments which I had made some years before.

I owe an apology to the Society and to Mr Duncan for the delay which has occurred in the fulfilment of that promise. The experiments which I have now to bring before you were begun in March and completed in June of the present year. At all these experiments I have myself been present during the whole period of each, and I have taken every precaution to make the test as reliable as it possibly could be. I have been ably seconded in this endeavour by my assistants Mr Hare and Mr Baily. The experiments during March were made with the co-operation of both these gentlemen, the later experiments by Mr Hare and myself. I feel very specially grateful to Mr Hare for the great trouble he has taken and the earnestness with which he has carried out the investigation. Mr Baily's departure from Edinburgh in May prevented him from sharing equally with Mr Hare and myself in any credit there may be in bringing the experiments to a conclusion. I have thought it right to bring all the flasks used before the Society in order that the Members may have ocular demonstration of the results obtained.

I think it will add to the clearness of what follows if I divide this paper into three parts, describing in turn the spray, the preparation of the alterable fluid, and the experiments performed with these two factors.

The spray employed was one of Marr's, which belonged to the late Dr Lyon of Glasgow. It possesses a high pressure safety valve, and is heated by a powerful spirit lamp, on the model of that which I introduced some years ago, and which has since been adopted, and, without my permission, patented by Mr Marr.

This spray was chosen because it is to some extent representative, as it is the one now used by Professor Lister.

A series of observations was first made as to the time it could safely be trusted to play actively, and the strength of the spray cloud it produced. It was found that it would play for 76 minutes, and that the strength of the cloud produced varied from 1 to 25 up to 1 to 29; this variation depending on the amount of 1 to 20 carbolic acid solution present in the receiver. If that receptacle was full, the strength was 1 to 25; if, on the other hand, it was nearly empty, the increased difficulty in raising the column of lotion

diminished the strength of the cloud produced to 1 to 29. As, however, in all this series of experiments the receiver was maintained at least half full, the weakest spray cloud we had here to deal with was one of 1 to 27. The acid used in making the solution was the pure crystalline phenol of Bowdler & Bickerdike.

Authorities¹ are agreed that a solution of 1 to 30 is sufficient to destroy organic life. In using a carbolized vapour of 1 to 27 we were within the recognised standard.

The fluid used in the experiments I have now to describe was a *half per cent. solution* of Darby's Fluid Meat (sp. gr. 1002), which forms a clear and highly refractile medium, which is peculiarly capable of maintaining the growth of organisms, and gives a distinct cloudy reaction when infection does occur. It was first filtered, and then steamed (at 212° F.) in the steam sterilizing apparatus devised by Koch, for 30 minutes on each of two consecutive days. This process is found amply sufficient to preserve the great majority of nutrient fluids, and has the special advantage of preventing any evaporation, so that the experimenter can work with a sterile liquid of known bulk and specific gravity. To the importance of the latter point I shall subsequently refer. The liquid thus prepared was then decanted into glass beakers supplied with cotton-wool plugs, the whole of which, with their plugs, had been previously sterilized by super-heating in a hot-air chamber (320° F.) for one hour.² To insure complete sterilization of the charged beakers, and to obviate the risks of contamination which necessarily accompany the decanting, the whole were next subjected to an additional 30 minutes' steaming on each of two succeeding days.

All the beakers so prepared were now incubated for four days at a temperature of 95°–102° F., and under this incubation a small proportion of the beakers became cloudy. These were rejected from the experiment. Beakers of three sizes were employed, of 3¼, 2, and 1 inch diameter respectively, and they were charged in the following proportions:—

| | | |
|--------------------------------|-----------|----------------------|
| In the large beakers (3¼ in.), | . 150 cc. | } nutrient fluid. |
| In the medium beakers (2 in.), | . 25 cc. | |
| In the small beakers (1 in.), | . 10 cc. | |

The experiments may for reference be divided into four series. The 1st and 2nd series were performed in the Surgical laboratory, the 3rd and 4th series in the Surgical lecture theatre in the New University Buildings. Beakers of all three sizes were used in the

¹ Vide Lister—*Dublin Journal of Medical Science*, August 1879. Watson Cheyne—*Antiseptic Surgery*, p. 26. Beatson—*On Lister's Antiseptic System*, part viii. p. 21. Miquel—*Les organismes vivants de l'atmosphère*, p. 293.

² See Koch's method in Ziemssen and v. Pettenkofer's *Handbuch der Hygiene: Fermente und Mikroparasiten*, p. 296; also Klein in *Practitioner*, March 1883.

1st and 2nd series, beakers of the large size only in the 3rd and 4th series. This alteration was made for two reasons.

1st, That from experiments on the amount of carbolic solution which fell into the smaller sizes of beakers there was a possibility that the growth of organisms might be checked in consequence of the nutrient fluid becoming carbolized.

2nd, Because Mr Duncan in his paper laid special stress on the impossibility of preserving beakers of the largest size ($3\frac{1}{4}$ in.) aseptic for more than 20 minutes.

The beakers and spray were arranged on a table in each of the experiments, as is seen in Diagram 1.

In all cases control experiments were conducted alongside of each spray experiment.

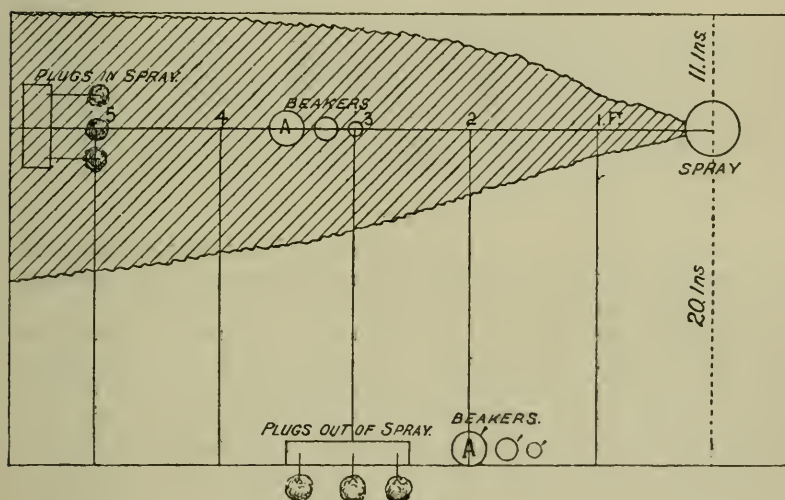


FIG. 1.

Each experiment in the 1st and 2nd series, as is shown in the diagram (Fig. 1.), consisted in uncovering beakers of three sizes, and in the 3rd and 4th series beakers of one size only, respectively in and out of the spray for the same length of time, at a distance of three feet from the mouth of the spray. The plugs of the three spray beakers were suspended in the spray cloud; those of the non-spray beakers out of the spray, with their lower ends directed downwards and covered by a cloth soaked in a one per cent. solution of mercuric chloride, to obviate, so far as possible, contamination through the natural tendency of atmospheric germs to gravitate. The hands and arms of the manipulator and every other object, which even approached the area of the experiment, were carefully disinfected by soaking in the same solution.

The results of these experiments can best be grasped by reference to the 2nd diagram (Fig. 2).

| | In Spray | Out of Spray | In Spray | Out of Spray | In Spray | Out of Spray | In Spray | Out of Spray | |
|--------------------------|----------|--------------|----------|--------------|----------|--------------|----------|--------------|-------------------|
| 1 ST SERIES. | | | x | | | | | | IN LABORATORY |
| | | | | | | | | | |
| | | | | | | | | | |
| 2 ND SERIES. | | | | | | | | | |
| | | | | | | | | | |
| | | | | | | | | | |
| 3 RD SERIES. | x | | | | | | | | IN CLASS ROOM. |
| 4 TH SERIES. | | | | | | | | | |
| DURATION OF EXPOSURE.... | 10 MIN. | 20 MIN. | 30 MIN. | 40 MIN. | 50 MIN. | = CLEAR | = CLOUDY | | |

FIG. 2.

It will be seen that of 40 beakers exposed out of the spray, 38 became turbid; the two which remained clear being of the smallest size—1 in. diameter.

Of 40 beakers exposed in the spray 38 remained clear, and the two which became cloudy were of the largest size—3¼ in. This coincides exactly with Mr Duncan's statement of the relatively greater difficulty of preserving large sized beakers aseptic. In looking over the laboratory note-book, however, the following entry, which explains the failure, is found opposite the description of the experiment in which one of these two clouded beakers was exposed:—

"April 1st, 4.20 p.m. Expt. commenced.—Lamp was not turned on at beginning of this experiment. Mistake discovered and remedied at 4.23 P.M." This note was jotted down at the time, and came to have a peculiar significance, when, on the fifth day after the experiment, the beaker in question showed signs of active organic life.

As the foregoing series of experiments was performed merely with a view to testing the results of Mr Duncan's series, I have thought it well to compare diagrammatically the results obtained in each. In this diagram (Fig. 3) the deeply shaded parts represent cloudy beakers, the clear parts unaltered beakers, and the proportions are drawn to scale, ¾ in. being allotted to each beaker; and by this method of comparison the wide difference between Mr Duncan's results and my own is at once apparent. It would appear that I have worked either in a much

more impure atmosphere than Mr Duncan, or with a medium which is more putrescible; but in either case the action of the spray as an antiseptic agent is the more remarkable.

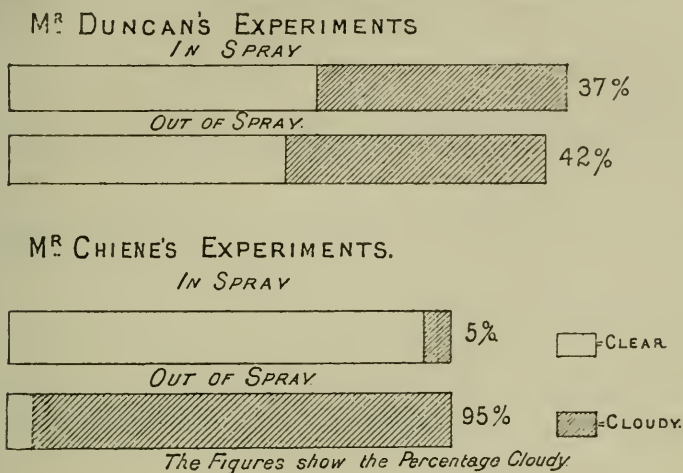


FIG. 3.

The striking results obtained in these experiments may be explained on one of four suppositions:—

1. The purity of the atmosphere.¹
2. The mechanical action of the spray, displacing and driving away the causes of putrefaction from the mouth of the beaker.
3. The chemical action of the condensed carbolic acid solution from the spray which falls into the beaker and renders its contents incapable of supporting life.
4. The germicide action of the spray vapour which mixes with the air at the mouth of the beaker and destroys the vitality of the germs suspended in it.

The first hypothesis at once falls to the ground, for the results of the control experiments with flasks exposed outside the spray show that we had to deal with a densely germ-laden atmosphere, and it could only be tenable under such circumstances as those obtained by Tyndall in his experiments with glacier air, which he found to be totally germless.

The mechanical theory falls to the ground when we remember that sprays of salicylic and boric acids have been found ineffectual. The same result occurred in some experiments tried by Mr Hare during the last few days with a spray of boiled distilled water.

The chemical theory is strongly opposed by Mr Duncan, who states that in his experiments the amount of carbolic acid which fell into the beakers may be left out of account, as it did not prevent the majority of them becoming septic. In addition we

¹ See Tyndall, *Floating Matter in Air*, pp. 294-296.

have the authority of Miquel for stating that a solution of less than 1 to 31 will not, with certainty, prevent putrefaction.

A series of experiments was performed with reference to this point, and I found that in the largest sized ($3\frac{1}{4}$ in.) beakers, enough carbolic acid found entrance in the course of 30 minutes to bring up the strength of the solution in the beaker to 1 in 3000.

Can such a weak solution have any effect on the putrescibility of the solution of Darby's Fluid Meat? or are the effects due to the destructive power of the spray acting directly on the organisms?

I am now occupied in an attempt to solve this rather intricate problem, and I shall venture to indicate the way in which I think the question must be approached.

In the first place, I was anxious to learn what happens to the carbolic solution which falls into a beaker standing in the spray. Does it diffuse itself in the liquid, or float, or sink? To settle this I have called into use one of the most beautiful and delicate reactions which chemistry affords us, viz., the violet coloured reaction which occurs on the combination of carbolic acid with a solution of perchloride of iron.

In my first experiment I used a mixture of water, with glycerine and perchloride of iron, a liquid of high sp. gr. On turning the spray upon this a beautiful purple layer, very delicate in its thinness, was soon apparent floating on the surface, and it continued to become thicker and more distinct whilst the spray played upon it. In the next experiment the liquor (B.P.) was used in the strength of one drop to the ounce of water, with a sp. gr. infinitesimally above that of water itself. Here the result was very different. Whilst the spray was playing upon this liquid, minute violet drops were seen to form at the surface, and to slowly gravitate downwards to the floor of the beaker. At the end of half an hour's experiment a faint violet cloud occupied the lower strata of the liquid in the beaker. Hence the subsequent behaviour of the carbolic solution which falls into a beaker depends greatly upon the sp. gr. of the contained liquid; and the practical indication from this is to employ a fluid of sp. gr. as near as possible equal to that of the solution thrown in the spray cloud, so that diffusion may be rapid and complete, and no local action of concentrated lotion will occur at any part of the beaker.

In the experiments already detailed beakers of large size have been preserved aseptic, after exposure, for periods of 10, 20, 30, 40, and 50 minutes each, and still remain clear, but these experiments throw no special light upon the question of how the spray exerts its now undoubted power. It seems to me that the crucial test of this matter will be the momentary exposure of large beakers under the spray in an atmosphere so densely germ laden as to insure that some pass into the beaker after traversing the spray, the period of

exposure being at the same time so short that the amount of carbolic solution which enters shall be absolutely infinitesimal.

Another method of research which commends itself is the attempt to exclude the spray altogether from the sterile nutrient material, and I am attempting to accomplish this by using solid instead of liquid media, the beakers containing which can be inverted or placed at any angle, so as to prevent the ingress of the spray cloud.

Such an arrangement I now show you, which consists of a tumbler with a layer of Koch's nutrient peptonized gelatine, enclosed and sterilized in a beaker by a method devised by Dr Woodhead and Mr Hare. These beakers can be exposed in two groups with like conditions, as, for example, opening towards a cross draught, which in one case traverses the spray cloud, and in the other does not, and the results compared. The results obtained in some preliminary experiments with this method have been of a very encouraging nature, and it seems that for future experiments the greater convenience of the solid material, which is equally clear and equally putrescible with the liquid media, will banish the latter altogether from use.

Mr Duncan's experiments have been adopted by himself and several of our colleagues as the basis of a modification in their practice; and we have already heard (*Ed. Med. Journ.*, May 1884) of the practical results of the change.

I wish it to be distinctly understood that in this paper no reference whatever can be made to the question as to whether we should or should not continue the use of the spray, as practical surgeons; for though it is true that the changes of method to which I have referred were the outcome of Mr Duncan's experiments, it is with those experiments alone that we have now to deal, and not with deductions from them, and it is their accuracy alone which I have put to the test. Clinical results, by whatever method obtained, are doubtless the only basis upon which the merits of various methods can be decided, but with the present inquiry they have nothing to do.

The point in Mr Duncan's paper which seemed to me of special interest, and on which he laid special stress, was the size of the beakers used. I have experimented with 20 large beakers, and I have been able to preserve 18 of them under the spray without any subsequent alteration. The two beakers which went wrong were exposed respectively for 10 and 20 minutes, and I have already given what appears to me a sufficient explanation of the failure in one of those beakers.

Mr Duncan did not succeed beyond a period of 20 minutes in keeping "a single example unclouded." Every beaker beyond 20 minutes which I have experimented with is now unchanged before you.

I trust that I have to-night demonstrated that the experimental

research of Mr Duncan cannot be accepted as a satisfactory scientific basis on which to found any alteration in practice.

The use or disuse of the spray may or may not be justified on other grounds, but it certainly cannot now rest on the results brought by Mr Duncan before the Society in February 1883.

Mr Duncan said it was necessary that he should make some observations upon a paper founded on some experiments he had made a year and a half ago, and on which he had founded, as a quasi-scientific basis, the change in practice he afterwards brought before the Society, and the results of which were known to most of the members. *Mr Chiene* had been good enough, a few days ago, to inform him of his experiments, and he had intended to have gone to witness them, but engagements out of town had prevented. He had not even had time to study the details of his own experiments. Comparing *Mr Chiene's* experiments with his own, the conclusion that one would naturally come to was that one or other must be absolutely right, and in that case that the probability would lie with the positive evidence. There were sources of fallacy in regard to the negative evidence which did not lie in the positive. Certainly had his results turned out with an amount of non-putrescence amounting to 95 per cent., he should have regarded this as a triumph for the spray. But there were some points in those experiments worth noting. The first point was with reference to those beakers which were not under the spray. They would see, in comparing *Mr Chiene's* results with his, that of those which he had not put under the spray 58 per cent. remained free from putrescence, but of *Mr Chiene's* only 5 per cent. remained free. His own number was made up largely of small beakers. All the large ones went wrong. It was a remarkable fact that a very much larger proportion of those not exposed to the spray remained clear with him than with *Mr Chiene*. Looking to that point, he should claim that the manipulative skill of himself and those engaged with him was, at all events, sufficient for protective purposes. The personal equation might therefore be eliminated. What further conclusion was to come therefrom? Taking it for granted that their experiments were both to be looked on as correct, the conclusion must evidently be either that the atmosphere in which he worked was purer, or that the soil was less capable of germinating organisms. The soil was apparently the same—*Darby's* fluid meat. They were reduced to the conclusion that, given an equal manipulative skill and a similar soil, the atmosphere in which he worked was more pure than that in which *Mr Chiene* did. But then comes the question of the experiments with those under the spray. *Mr Chiene* had a proportion of those that kept free from putrescence overwhelming compared with his. How was this to be accounted for? On the same principle, that the experiments are strictly comparable (they are

not identical), and that there was equal manipulative skill, the deduction was that either Mr Chiene worked in a purer atmosphere, or that his soil was less favourable to germs. But, as they had seen, this view of the soil could not be entertained. The only explanation he could see was that there were fewer germs in his (Mr Duncan's) room, but more capable of resisting carbolic acid. They knew that germs resisted very variously. In Watson Cheyne's experiments they had seen that some germs flourished in 1 to 300 carbolic acid, others were killed by 1 to 500. If this conclusion were correct, it necessitated the supposition that the spray was effective in killing so many germs—that a certain number of germs were capable of being killed by the spray, but others were not. At that moment it seemed to him difficult to escape from this conclusion, that the spray was more effective than he had thought from his experiments. Another suggestion, which might have a bearing on this point, was that they were working in a small room in which the spray was perpetually going, so that the atmosphere in it might have been rendered, so far as carbolic destruction of germs was concerned, very free from all the germs which were capable of being killed by carbolic acid, and the probability that they were working with germs not capable of being killed by carbolic acid was strengthened by the fact that his experiments were done in his side-room in the Infirmary, while Mr Chiene's were done in his laboratory in the University. It was a mistake to suppose that he had ever denied the power of carbolic acid to kill germs. He pointed out in the very paper to which Mr Chiene referred that carbolic vapours might kill germs if sufficiently strong. Well, then, there was another point in connexion with this which was of some interest. Supposing the spray acted in this way, how did it do it? That was a point on which he could not give an accurate decision. *A priori* one would be inclined to suppose that in the figure on the board the mechanical blowing away of the germs had more effect than Mr Chiene supposed. Although some experiments had led him (Mr Duncan) to suppose that the centre was the safest place in the neighbourhood, and its margin by a long way the most dangerous, yet he had no doubt whatever that a pure mechanical steam spray without introduction of an antiseptic was not thoroughly effective mechanically. There remained, therefore, the introduction of carbolic acid into the beakers, or that the spray destroyed germs on its way. The introduction of carbolic acid into the beaker might prevent germination, but he did not believe that in his own experiments this was an important factor, because he found that beakers went wrong after the spray had been pumped over them for a long time. It might have a greater effect in the smaller quantity of fluid which Mr Chiene used. But granting that Mr Chiene's experiments proved that the spray acted as spray, it could not be regarded as reliable.

If they considered the matter *a priori*, reason must lead them to see that such must be the case. If they took a stocking-wire and dipped it into a septic fluid and then into a putrescible beaker, no one would suppose that the amount of carbolic acid it got in passing through the spray would be sufficient to prevent germination. A dirty finger passed through the spray into a putrescible beaker would make it decay. The element of time was thus an element of considerable importance. It was necessary that the spray be so manipulated that the germs should be kept in connexion with it for some time before they dropped into the beaker, and in this respect there might be something in the great depth of Mr Chiene's beakers. Therefore it seemed to him that what benefit was derivable from the spray, and it must be more considerable than he had thought, was a combination of the germicide properties of carbolic acid, and of the mechanical properties of the spray. These questions, which were full of interesting problems in every direction, and well worthy of elucidation, he hoped in some directions further to follow out. Meantime he had no regret that his experiments had served as a quasi-scientific basis for a change in practice. The result of the change was in itself, to his mind, quite sufficient to justify the change in practice. Even were he now to be convinced of the power of the spray to destroy immediately germs in the atmosphere, he should not return to it because of its inconvenience, and because they had other methods of treatment which were quite reliable. His results with his new mode of treatment were very much better than any he had got with gauze, mackintosh, and spray.

Dr Black, in the course of a few remarks, detailed the improvements that had occurred in Manchester Royal Infirmary after the introduction of the antiseptic system by Mr Lund.

Mr Joseph Bell felt that the Society was much indebted to Mr Chiene and Mr Duncan for their admirable dialectics. For his own part, when Mr Duncan showed them that he was able to keep 58 per cent. of putrescible beakers free, it convinced him that the danger from falling germs was not so great as they had been taught to believe. Mr Duncan's experiments, he thought, showed that it was less important to work at the atmosphere than at the wound. He had not himself been making any experiments with the spray, but his practical experience was that it could be done without. He had treated his wounds of late with the bichloride of mercury solution, recommended by Koch, and he found that they got on better and required to be dressed much less frequently than formerly when the spray was in constant use. The application of the spray during the operation was, he thought, a cause of irritation and a main factor in the production of excessive serous discharge. His results were now much better, he was more comfortable in mind about his cases, and they had got rid of the

spray, which in itself was a very great nuisance. True, they used another instrument in its place—an irrigator, which was more convenient and, perhaps, more effectual. It was devised by his late house-surgeon, Mr Gardiner, and consisted of an Ingram's enema syringe with a small perforated nozzle. By means of it Koch's solution was injected into every part of the wound at the conclusion of an operation. Notwithstanding this change of practice he was as strongly as ever an aseptic surgeon, though he did not now feel it necessary to keep the air immediately over the wound aseptic. One thing struck him in reference to the unprotected beakers in Mr Chiene's experiments, and that was their nearness to the spray. If the mechanical theory of the effect of the spray had any truth in it, it was not improbable that germs may have been blown directly into the beakers.

Mr Cutheart thought that to test the question of how the spray acted, it might be well to allow the spray to play over the unprotected beakers, after their period of exposure was over. If a sufficient quantity of carbolic acid from the spray entered the beaker to stop or modify the putrescence, its effects could in this way be tested by comparing results with beakers which had not been subjected to the carbolic spray after exposure.

Mr Miller had listened to Mr Chiene's paper, and had witnessed his demonstration with very great satisfaction, because he was still one of the few surgeons who supported the spray. Mr Chiene spoke of experiments that were being made to bring this matter to a more thorough demonstration. He would suggest to him that if the mechanical theory was not the correct one, that the best proof he could give them that the germs really found their way into those fluids, but unable to do harm, he might show them the dead germs. Whatever be the explanation, he took it that Mr Chiene had demonstrated to them that the spray was more beneficial than they were led to believe last year. With regard to Mr Duncan's reply that evening, he would wish to say this, that they would always find that when a man had got something he wished to prove, it was by no means difficult for him to prove it. If any surgeon with ordinary talents—and Mr Duncan had extraordinary—devoted himself to the idea that he could treat surgical cases well by any special method, he could readily do it. He remembered very well that when he was house-surgeon with Mr Spence they got remarkably good results without any dressings whatever, but this was not due to the absence of dressings, but to the great care taken by Mr Spence. Mr Gamgee's results with dry dressing was another example. As to the kinds of cases that were good tests of the value of the spray, he did not think amputations and such like were, but rather abscesses and other cases containing fluids that were capable of putrescence. Take, for example, abscesses connected with carious bone. Would the same proportion of cases

do well without the spray as with it? He thought not. He did not look with the same pleasure on Mr Duncan's paper. A sort of contempt for germs and sprays had been fostered since its publication. His dressers, he found, had not the same respect for the spray as he himself had, and if any case were left to them it was almost certain to go wrong.

Dr Woodhead said that in reference to what Mr Duncan had said about the personal equation, he left out of account one factor which he (*Dr Woodhead*) should have considered of importance—the difficulty there was in purifying large beakers and keeping them pure. Any one who had worked at this subject would find it much more difficult to carry on experiments with those flasks than with small ones. A very much greater amount of manipulative care was necessary. As to Mr Cathcart's suggestion, he had thought of the same thing, slightly varied, when Mr Chiene was speaking. When these flasks had been exposed to the spray for a time, and had received a certain amount of carbolic acid, they might be exposed for a time without the spray. Germs entering would then probably be prevented from germinating by the layer of carbolic acid on the surface of the fluid. As to what Mr Bell said in reference to the position of Mr Chiene's beakers, he should like to know from Mr Duncan where his unprotected beakers stood in relation to the spray.

Mr Duncan said they were placed at various distances, sometimes on the same table, sometimes on another, but usually behind the spray, so that they would not be influenced in any way by the currents. There were many differences between Mr Chiene's experiments and his own, viz., the strength of the spray, the size of the beakers, the quantity of the fluid in the beakers, the treatment of the plug. These might or might not be important. He had made a suggestion only on the supposition that they were not important. He would point out to *Dr Woodhead* that to render aseptic the large beakers Tyndall's plan was very effective.

Professor Chiene thanked the Society for the manner in which his paper had been received. He was not prepared to enter on the position that Mr Duncan had taken up. He would peruse what he had said. He thought, however, that whatever germ Mr Duncan had in his side-room, he (*Mr Chiene*) could show a similar germ in his laboratory, which had been fitted up for the purpose of studying the connexion of germs and disease. It was a richly germ-laden room.

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